

Aquatic Ape Theory: Sink or Swim?

a scientific critique by Jim Moore

"False facts are highly injurious to the progress of science, for they often endure long; but false views, if supported by some evidence, do little harm, for everyone takes a salutary pleasure in proving their falseness: and when this is done, one path toward error is closed and the road to truth is often at the same time opened."

Charles Darwin, 1871 *The Descent of Man and Selection in Relation to Sex*, Chapter 21, page 909.

The Aquatic Ape Theory (often referred to as the AAT or AAH -- I'll generally refer to it as the AAT/H on this site) says humans went through an aquatic or semi-aquatic stage in our evolution and that this accounts for many features seen in human anatomy and physiology. Using the principle of convergent evolution, it says that life in an aquatic environment explains these features, and that a transition from ape to hominid in a non-aquatic environment cannot.

This web page offers a critical examination of the Aquatic Ape Theory, treating it as a serious scientific theory. It is one of the few online sources that does so. There are many other web sites which deal with it, but they do not offer the critical examination which any theory needs to be given.

Why have I done this site?

Two reasons:

1) Because AAT/H proponents ask why don't people take their theory seriously as science, and the way you take a theory seriously as science is to examine it for accuracy and criticize it where it falls short, because...

2) "Valid criticism does you a favor" Carl Sagan, page 32 of *The Demon-Haunted World* (1995).

I am doing what many AAT/H proponents -- including its principal proponent, Elaine Morgan -- have repeatedly claimed they want done: treating the AAT/H as befits a serious scientific theory.

Accepting any new theory uncritically is foolish. When doing a critique of *any* theory of human evolution, you check the facts the authors use to support the theory.

All scientific theories need to be examined for accuracy; it's an essential component of the process of science. I'm afraid that when the Aquatic Ape Theory is examined, it does not fare well. The AAT/H is built on many supposed facts which, when examined, do not turn out to be true. Perhaps the kindest thing would be to ignore it, but I am not that kind.

Instead I've begun an ongoing response of pointing out errors of fact, errors in theoretical understanding (which, though critically important, is more problematic because a lot of people seem to think this is waffling), and urging the theory's proponents to respond to valid objections to their theory.

Examining the Facts: A necessary process of science

It shouldn't have to be pointed out that when someone produces a theory or hypothesis and calls it science, it is perfectly legitimate to look at the facts used to support it. While I've been doing this with the facts which are supposed to support the AAT/H, I've found some people don't think this is a legitimate thing to do. Well, it's not only legit, *it's absolutely necessary*. That's how science works; without this process, science is simply dueling beliefs, with no progress. The process of examining evidence and discarding inaccuracies is crucial to the ongoing process of science.

There are a number of claims made by AAT/H proponents, and tracking down the source material which supposedly supports these claims is not always easy. In a dramatic departure from the way science is supposed to work, references for specific AAT/H claims are not always available; many times the author(s) involved simply make a claim without any reference to a source of evidence for the claim. For instance, during the period I put together most of the evidence for this site, chief AAT/H theorizer Elaine Morgan was posting regularly in the sci.anthropology.paleo newsgroup, and claimed to be willing to supply references for her written AAT/H claims, but she proved to be reluctant to provide these references to people who have a past history of actually reading the source and reporting back what it really says. Others -- who didn't engage in this sort of "anti-social" behavior -- reported they have no problem getting references from Morgan.

Nevertheless, a number of the claims made by Elaine Morgan and other AAT/H proponents have been tracked down and examined. The results are not

encouraging. Quite frankly, when I first read the work done on the AAT/H, I saw some big holes in the reasoning, but I did think that the evidence which was (sometimes) given was probably accurately and fully reported. When I started looking these things up, however, I found that I was wrong on that count -- the AAT/H has proven to be a hotbed of those "false facts" Darwin referred to.

What makes me think I know what I'm talking about here?

I don't have any formal credentials in evolutionary science, so how can I expect you to believe what I tell you instead of what AAT/H proponents say? Leaving aside the fact that all the major AAT/H proponents also lack such credentials, you simply look at whether or not what I say matches actual, known facts. On this site I try to provide references for my statements, and appropriate, unaltered quotes from relevant sources to back up my facts. The more curious or enterprising amongst you may wish to read further about some of these features, and you'll find that easy to do by simply going to a library and picking up one of the books or journals I mention. If you try to do the same with most pro-AAT/H accounts, you'll find one of several problems: they often don't give a reference for a statement or quote at all; when they do the reference is often incomplete (just a name with no clue as to what publication or year the info is supposedly from; sometimes even a wrong name). On this site, I've tried to tie as many of my statements to specific references as I can; at present this is still incomplete and will be rectified during my updates. Pro-AAT/H accounts also commonly use quotes which are altered to "say" something the quoted person didn't actually say; they leave out relevant material from the same source which contradicts the AAT/H position; or they simply claim the source says the opposite of what that source actually does say. You won't find those problems here. If you do find an error on my site, please contact me -- there's a feedback link on most pages.

I got started on this subject through reading and posting in various science newsgroups during the early days of the web. I've got a knack for library research and I like the detective work that's needed to find an appropriate source of information. I've also had an informal background in anthropological research, helping my late wife, Nancy Tanner, with her research on human origins, matriliney, and social organization. This entailed finding, reading, and learning to understand a lot of info that I didn't know, such as molecular studies. In the process, I've learned that my knack for research also includes an ability to translate some pretty heavy technical jargon into English, and explain some complicated subjects in relatively simple terms. This ability to ferret out information sources from often obscure clues came in handy when I started to examine the evidence offered for the AAT/H. The references for AAT/H statements, when they are given at all, are often maddeningly

incomplete or misleading. For instance, it took some hunting to find the source for a quote when the quote supposedly came from a "famous authority during a television programme" (it was actually a 1929 book by Prof. Frederick Wood Jones). Another time the source of information for a claim was said to be a 1979 book, with an author mentioned. Finding the ref then started with finding that book, then finding the one page out of hundreds (which wasn't given) that referred to the *actual* reference which contained the info (a 1956 article), then finding the article which contained the info, which, it turned out, didn't actually say what the AAT/H proponent claimed it did. To complete the critique of that one AAT/H claim (about seal sweat) also required finding yet another article, and a total of perhaps 6-8 hours of actual research time. I'll guarantee you the original one paragraph AAT/H bogus claim didn't take that long to churn out... and people wonder why anthropologists don't spend their time and meager research grants chasing down AAT/H claims.

So I guess I'd say my qualifications for this work are 1) a knack for library detective work, 2) an ability to learn basic scientific precepts (anyone should be able to do this one), and 3) being just a little bit nutty, cause those examples I just gave show you've got to be a little crazy to do it.

As a note, let me state that I am *not* the Jim Moore who is a primatologist at UCSD, nor am I the Jim Moore who wrote and cowrote several books on Darwin, nor the Jim Moore who's an anthropologist at CUNY. Although I'm not them, from what I've seen of their work, I wouldn't be insulted to be mistaken for any of them. I hope they feel the same.

I'd like to thank the posters in the old sci.anthropology.paleo newsgroup, in particular Phil Nicholls and Alex Duncan, for pointing out many false facts and methodological errors in the AAT/H's arguments. Their research was helpful to me many times. This site is a partial update and redo of a site I had up several years ago, and I will be continuing to update it whenever I have the chance. In surfing while rewriting and adding to this site, I've noticed several AAT/H boosters make the rather odd criticism that my previous site was old, as if that somehow made the facts therein wrong. Although many of the facts posted on this site were first posted on my old site in 1995, they are still valid. The facts have not changed. The diving reflex has been present in terrestrial mammals for centuries, it was in 1995, and it still is, despite the AAT/H claim to the contrary. Salt hunger has been present in humans for centuries, it was in 1995, and it still is, despite the AAT/H claim to the contrary. The infant swimming response has been present in terrestrial mammals for centuries, it was in 1995, and it still is, despite the AAT/H claim to the contrary. Hymens have been present in terrestrial mammals for centuries, they were in 1995, and they still are, despite the AAT/H claim to the contrary. I could go on, but you'll

see all these facts as you go through the site. Suffice it to say that there have been valid objections to the AAT/H since its inception, and there still are. Established facts, as opposed to the AAT/H's many "false facts", do not change.

I've also noticed a criticism that I simply engage in *ad hominem* attacks on AAT/H proponents, in particular Elaine Morgan. This is a peculiarly funny sort of criticism, since it is in itself a variation of an *ad hominem* attack, that is, instead of attacking *the accuracy of what I'm saying*, it attacks *how* I say it. This is a classic logical fallacy, one of several that AAT/H proponents often engage in -- there's a link to a site on logical fallacies on my links page. The thing is, I go after *the accuracy of what these people say*, and, yes, I have an aggressive style. But pointing out that someone made an error, or altered a quote, or said that a scientist said one thing when they actually said the opposite, is not an *ad hominem* attack. Frankly, it's doing them an enormous favor, as Carl Sagan stated in that quote. (This does, of course, assume they're trying to do a good, accurate job at what they're saying.) It may not sit well with everyone, but quite frankly, I find it *really* annoying when good science is taken to task for not accepting a theory which is so full of holes and mistruths, and which is argued for so dishonestly. And, by the way, not only are honest scientists harder on each other than I am on Morgan, Verhaegen et al., any honest scientist is harder on their *own* work than I am on Morgan, Verhaegen et al.

Contents

Navigation note: You can navigate through this site's pages by simply following the "Next" button at the bottom of the pages, or you can use this table of contents to go directly to a page. I've made the assumption that some of these pages (the linked but indented pages below) will be reached from here or through the "AAT/H claims and the facts" page, since they contain info about questions raised there. Navigating back from those pages must be done with your browser's Back button. If you find this to be a problem, let me know. This is a wide-ranging site, and I want to make navigation through it as sensible as possible.

Aquatic Ape Theory: Sink or Swim? (this page)

Why have I done this site?

Examining the Facts: A necessary process of science

What makes me think I know what I'm talking about here?

[What is the Aquatic Ape Theory \(AAT/H\)?](#)

Who thought up the Aquatic Ape Theory?

[Aquatic rhinos?](#)

[Vernon Reynolds' Conclusions from the Valkenburg conference and The Aquatic Ape: Fact or Fiction?](#)

What about credentials -- do they matter?

[General Problems with the AAT/H](#)

The method of the Aquatic Ape Theory (AAT/H)

[Convergent evolution](#)

Problems with the idea itself

Problems with the presentation and argumentation of the AAT/H:

Logical fallacies

[Definition of savannah](#)

Sexual selection and the AAT/H

[Natural and Sexual Selection](#)

ZING!ability, the shifting target

Perhaps the oddest problem with AAT/H proponents' methods

[The AAT/H characteristics list](#)

The AAT/H leaflet list

The AAT/H leaflet list...annotated

[Relevant Questions for the Aquatic Ape Theory](#)

Actual ubiquitous convergent features of aquatic mammals

[AAT/H claims and the facts](#)

[Hair and "Hairlessness"](#)

[Aldosterone, water, and bipedalism](#)

[Fat and the AAT/H](#)

[Breath-holding, the diving reflex, the larynx, and the AAT/H](#)

[Tears and the AAT/H](#)

Homeostasis

[Salt glands](#)

[Salt and the AAT/H](#)

[Mating Postures and the AAT/H](#)

[Body Temperature and the AAT/H](#)

[The "Swimming Babies" reference](#)

[What about predators?](#)

[Skin, sweat, and glands](#)

[Can AAT/H proponents research be trusted?](#)

[Wegener, Continental Drift, and unaccepted theories](#)

[Why don't anthropologists mention the AAT/H much?](#)

[A brief critique of Morgan's 1997 book, *The Aquatic Ape Hypothesis*](#)

How to misquote Darwin

Other AAT/H theorists

[Alister Hardy's original "Aquatic ape theory"](#)

[Marc Verhaegen](#)

[The Omega-3 gang](#)

[Algis Kuliukas](#)

[Summary](#)

Internal inconsistency
Why is the AAT/H so popular?
The unpopular role of luck in evolution
Parsimony and Occam's Razor
Vagueness
Theory leading the data

[Objections \(to my critiques\) and Answers](#)

[My response to Elaine Morgan's response to me](#)

[Notes on BBC Radio 4 program, "Scars of Evolution" by David Attenborough -- 12 and 19 April 2005](#)

[Brain size and EQ \(Encephalization Quotient\)](#)

[Review/Critique of BBC/Discovery Channel documentary *The Aquatic Ape*](#)

[Review/Critique of Elaine Morgan's 2008 book *The Naked Darwinist*](#)

[Review/Critique of Elaine Morgan's 2009 TED Talk](#)

[List of Aquatic and Semiaquatic Mammals](#)

[Danakil Island and the Baboon Marker: two of the AAT/H's "false facts" which have indeed "endured long"](#)

[References](#)

[Excerpts from feedback](#)

[Links to other apropos sites](#)

What is the Aquatic Ape Theory (AAT/H)?

The Aquatic Ape Theory (aka AAT or AAH - I'll generally use the term "AAT/H" on this site) hypothesizes that humans went through an aquatic or semi-aquatic stage in our evolution, generally said to have occurred during the transition from the last common ancestor we shared with apes (LCA) to hominids (although some AAT/H proponents claim it continued on through virtually the entire span of human evolution). It claims that certain features are seen in human anatomy and physiology which are only seen in humans and aquatic animals and that these constitute proof that our ape ancestors went through an aquatic phase in their transition from ape to hominid. Using the principle of convergent evolution, it says that life in an aquatic environment explains these features, and that a transition from ape to hominid in a non-aquatic environment cannot.

Who thought up the Aquatic Ape Theory?

Note: there are links to more detail on several of the AAT/H proponents mentioned below; you may want to read them now, but they may make more sense if they're read after this page and the next one (on AAT/H methods and problems). You can also reach those pages from the links at the bottom of the opening page.

The original theory was done by [Sir Alister Hardy](#), a marine biologist (an expert on plankton), who late in his career in 1960, gave a talk at the British Sub-Aqua Club (a scuba diving club) and a month later published in *New Scientist* an article on that talk called "Was Man More Aquatic in the Past?" It presented most of the basic ideas, and definitely the method, of the AAT/H.

Desmond Morris then mentioned Hardy's theory with a 2 page write-up in *The Naked Ape* in 1967.

Elaine Morgan, at the time an Oxford grad in English and a TV scriptwriter, entered the scene in 1972 with the book *Descent of Women*, the idea for which she got from Desmond Morris's book. This was a pop book, with a pretty chatty style which seems dated now but was popular then, and it sold quite well. Looking back at it, I wouldn't call it particularly female-oriented (compare Morgan's notion of early female hominid behavior being the result of continual rape to more fact-oriented ideas like Tanner and Zihlman's idea of the selective power of sexual choice by females in hominid evolution), but Morgan presented it as "the" alternative to what she then called "The Mighty Hunter" theory.

Morgan followed up with *The Aquatic Ape* in 1982, *The Scars of Evolution* in 1990, and another that came out in 1994, *The Descent of the Child*. Morgan says the books after her first one are supposed to be "more scientific", but most of the statements in them still aren't adequately cited -- it's difficult or impossible to tie many of the statements in the text with the relatively few refs given in the back. This makes it extremely difficult to provide the in-depth critique AAT/H proponents demand to counter their theory (even though the theory itself lacks this depth -- a classic double standard). Morgan also has a creative hand when it comes to quotations, especially quoting out of context in the manner of creationists. This too is made difficult to discover by Morgan's habit of rarely giving sources for her quotes, and even more rarely giving page numbers. This is counter to scientific and journalistic practice, and despite Morgan's protests that providing these references makes a popular book unpublishable, doing so is standard practice in many popular, even best-selling, non-fiction books -- popular biographies, for instance.

Morgan has written another book on this subject (*The Aquatic Ape Hypothesis*, 1997); as a refreshing change this time, she has included references for some, but by no means all of her statements (this is perhaps to be expected since many of

those statements are false). I haven't yet checked the references for her quotes to see if this book is a change to her well-established pattern of using misquotation, but it does still use most of the AAT/H's established "false facts" to support her theory, including the thoroughly discredited material on fat, tears, the larynx, hymens, direction of the nostrils, etc., which this site covers. I've done a critique of this book here on my site (the link to it is on the first page). And in 2008 Morgan, bare months after saying she didn't have enough material for a sixth AAT/H book, brought out a sixth AAT/H book; I've critiqued that as well and that link is also on the first page of this site.

There have been a few other people who've done articles on the AAT/H, and they belie Morgan's claim that you can't publish academic articles on the subject. (Good academic articles seem to be another matter entirely.) Chief among them is Belgian medical doctor [Marc Verhaegen](#), who has done a number of articles. Although Verhaegen's articles do have references, they also contain statements such as claiming that [rhinoceros are "predominantly aquatic"](#), and several other howlers among their many errors.

There was a conference in Valkenburg, The Netherlands, on the subject in 1987 with both pro and con participants, and a book from that, *The Aquatic Ape: Fact or Fiction?*, shows the relative strengths of their positions. Although I'm sure any AAT/H proponent would disagree, the pro-AAT/H articles in that book are distinctly amateurish compared to the ones which don't support the AAT/H. It's a good book to take out and read sometime if you can find it, as it does cover the basic arguments and has some good arguments to the contrary.

A short note: as it happens, when I wrote the preceding paragraph, I was wrong that AAT/H proponents would necessarily disagree that the pro-AAT/H articles in *The Aquatic Ape: Fact or Fiction?* were amateurish. It turns out that Elaine Morgan agrees with me: "It is quite true that the style of the opponents is more professional." She excuses this inexcusable state of affairs with the true but untrue statement that "The opponents were professionals." This statement is true, but implies the others weren't, which ignores the fact that many of the pro-AAT contributors in that book were university-level professionals in scientific fields, albeit professionals who did unprofessional work. And if she was talking about style in some way absent any context (ie. ignoring substance) let me point out that I was referring to their articles' competence, not some matter of stylishness without substance.

One AAT/H proponent has been saying for some time that I should have the conclusions from the Valkenburg conference on my site, so I've done a short page on that: [The Aquatic Ape: Fact or Fiction? Conclusions](#). After reading it you might wonder why he wanted it done, but that will remain a mystery for the ages.

In recent years it's become popular to point to a German-language publication on an aquatic hominid past from well before Hardy's, by a German anatomist named Max Westenhöfer. As a note of historical curiosity this could be sensible, but there seems to be a thought that it somehow legitimizes the idea of the AAT/H. However, there is no indication that Westenhöfer's "aquatic human" work influenced anyone within the AAT/H community, much less outside it. This should not be surprising, since from the excerpts I've seen translated it was really chock full of wildly mistaken notions, and concludes, in the words of one translator (Patrick Beck), that *Homo sapiens* cannot be a close relative of primates, and even uses fiction as evidence, as in the statement "For example, to me the story of Beowulf's struggle with the dragon under water is a hint that humans perhaps did live and fight with such dragons in water". This, to me, is not someone I'd like to claim is on my side :).

P.S. One thing I found from posting on this subject in sci.anthropology.paleo is that Elaine Morgan

A) doesn't like it when you imply she is the primary proponent of this theory (she is certainly the best known and widest read); and

B) doesn't like it when you mention any claims made by AAT/H proponents other than her.

Go figure.

But this seems to be contagious; Marc Verhaegen now also often takes umbrage if you critique an AAT/H claim that he doesn't make himself. But then taking umbrage seems to be a specialty with him; his online method tends toward gratuitous insults, often as the sole content of his newsgroup posts, and continually reposting the same, non-responsive, paragraphs (earning him the nickname "macro-man" after the usual technique for doing that), and, starting from his very first online post in 1998, comparing his position to Wegener, Galileo, etc. These methods certainly don't help his argument, instead placing his online newsgroup contributions in the realm of the netloon.

[Michael Crawford and Stephen Cunnane](#) are in the forefront of a group of nutrition specialists who, over the past decade or so, have embraced the AAT/H as supposedly giving support to their goal of promoting DHA in modern diets (this is the one of the fatty acids, often referred to as Omega-3 fatty acids, required for proper brain growth and is dealt with in more depth later on this site). They're academics in the general field of nutrition and they are trying to do something good, but their AAT/H work shows them to be seriously ignorant of evolutionary theory. The good thing they want to do is push the addition of DHA into foods, such as milk and eggs, much as we have long added vitamin D to milk. This is useful because many people don't eat the kind of diet that would give them a proper dose of DHA or LNA, as our ancestors' diets would. The push for additional DHA in modern diets is overall a good one, in my opinion, but in trying to tie this laudable goal in with a dubious theory, they do themselves no favors.

[Algis Kuliukas](#) is a former graduate student who has been writing (mostly online) about the AAT/H for several years now, and can often be found promoting the idea that bonobos are bipedal 92% of the time they are in water. This statement comes from a 5-hour portion of a 3-day study he did for his masters thesis, and refers to a total of 38 seconds his bonobo group spent in water during that study period. This percentage is curiously, and wildly, at odds with all other studies, which creates the question of why it's so different. Although asking this "why?" question would be a normal part of any scientific observation and although I can readily think of several likely reasons for this difference, this obvious question appears not to have occurred to Kuliukas. This, sadly, seems to be the norm for him, and along with other indications, makes it seem unlikely that he will break the pattern of poor AAT/H research.

Kuliukas uses as a time period for the AAT/H essentially the same "non-time period" as Verhaegen -- the entire span of human evolution. He has also suggested (somewhat like Verhaegen) that hominids spent very little time in the water, but this supposedly "milder" view is actually more radical than Hardy's and Morgan's for reasons I explain in the page specifically about his ideas.

The BBC has done both a TV program (rebroadcast in North America in essentially the same form by The Discovery Channel) and radio program on the AAT/H, the TV program in 1998 and the radio program in 2005. Both are rather credulous (particularly sad to say of the radio program, which was done by Sir David Attenborough) and both are critiqued on this site: [critique of BBC Radio 4 program, "Scars of Evolution" by David Attenborough](#) and [Critique of BBC/Discovery Channel documentary *The Aquatic Ape*](#)

When honest scientists have facts pointed out to them which prove their line of evidence is wrong, they drop that line of evidence. AAT/H proponents rarely do, and even when they do, others -- both active proponents and casual aficionados of the idea -- often continue to use it. Sometimes the very same person who says they've dropped it continues to use it (as in the Morgan example below). But if some of this evidence *has* been dropped, why do I bother documenting its inaccuracy here?

There are several reasons:

1. Although some of these ideas are dropped by one AAT/H proponent, other AAT/H proponents continue to use them. For instance, while Elaine Morgan says she dropped the "swimming babies evidence" after her 1982 book, Marc Verhaegen has continued to use it (without noting that it is a response common to all infant terrestrial mammals tested).
2. Even though an AAT/H proponent may *say* they've dropped an idea, the same person may *actually* continue to use it. For instance, in the sci.anthropology.paleo newsgroup post quoted below (from 13 July 95) Elaine Morgan says she dropped the "evidence" about the direction of the nostrils after her 1982 book, but she subsequently began using it again online -- *starting 14 July 95: the following day(!)*. She's also using that info, as well as the salt, sweat, and tears info she said she'd dropped, in her latest book. Also online, we've seen Elaine Morgan sometimes publicly drop lines of evidence, yet in the next sentence she's claimed that the evidence is mounting:

From: Elaine Morgan in sci.anthropology.paleo on 13 Jul 1995:

"In The Scars of Evolution you will seek in vain for a mention of hair tracts or swimming babies or the direction of the nostrils. New evidence for AAT comes in faster than anything I have to keep in abeyance pending further facts."

Note: All these things, however, were stated in 1982's *The Aquatic Ape* (although Morgan had also falsely claimed her inaccurate statements about the evolution of the nose only appeared in her 1972 book).

From: Elaine Morgan in sci.anthropology.paleo on 19 Jun 1995:

"The nose - yes, I did once make the mistaken assumption that the nasal spine was in evidence prior to H. erectus. That was about twenty years ago and was never repeated. You really are scraping the barrel."

If they have indeed been dropped from the roster of AAT/H evidence between then and 1990, it sounds like evidence is disappearing rather than accumulating.

3. And finally there is that Darwin quote you saw as you entered this site -- "False facts are highly injurious to the progress of science, for they often endure long..." The AAT/H has provided us with a plethora of "false facts", and even those which have been dropped by some AAT/H theorists continue to pop up again and again. Far more than the theory itself, these false AAT/H "facts" are indeed "enduring long"; having to go back and painstakingly refute them each time they reappear is time consuming and debilitating, and so they are -- as Darwin pointed out over 100 years ago -- highly injurious to the progress of science.
-

What about credentials -- do they matter?

Not to me. Some AAT/H critics point out that AAT/H proponents don't seem to have any formal academic credentials which prepare them to theorize about human evolution. Alister Hardy, though a respected marine biologist, had no background in anthropology-related studies; Elaine Morgan was and remained a television scriptwriter; Marc Verhaegen is a medical doctor; Algis Kuliukas is a post-graduate anthro student; Michael Crawford and Stephen Cunnane are academics who specialize in nutrition. I don't think this matters one whit... as long as AAT/H proponents do the research that's needed to build a new theory. Unfortunately, to date none of them have.

Do scientists care about this lack of credentials? I think some of them do, and they're wrong. Does *science* care about formal credentials? I have a two word answer: Alan Mootnick. Alan Mootnick is a gibbon guy. He has no formal academic credentials; and he's entirely self-taught, yet he's highly respected. He writes both academic and popular articles, gives presentations at academic conferences, and has gained the respect of primatologists. How did he manage this? By doing the work. If you do the work; dig out real facts and report them accurately, and make logical conclusions from those accurately reported bona fide facts, a self-taught non-academic is listened to. Unfortunately, this process of digging out actual facts and reporting them accurately, and of making logical conclusions, does not describe the AAT/H research to date.

A reader pointed out to me in feedback from this site that another widely known example of someone who does the work well, without formal credentials, and is widely respected for it, is Richard Leakey (or the entire Leakey family, for that matter), and that another, perhaps less well known example is Kamoya Kimeu, one of the better known fossil hunters in east Africa.

Rhinos, Pigs, and selective evidence

There are four genera of rhinoceros, with five species: in Africa there are the black rhino and the white rhino -- neither is either black or white, by the way, the white rhino got its name from an Afrikaans word: *weit* ("wide"), referring to its mouth, and the black rhino either got its name from the dark soil it usually rolls in, or because it's slightly darker than the white rhino. In Asia there are the Indian, Javan, and Sumatran rhinos.

Rhinos enter into the AAT/H because some AAT/H proponents attempt to use them as examples of hairlessness due to an aquatic adaptation. What, you didn't know rhinos were aquatic? Well, that's because they're not, but a little problem like that isn't enough to stop a dedicated paradigm shifter. Some AAT/H proponents use wallowing in mud or water puddles as examples of aquaticness, for instance, and say that this degree of aquaticness is enough to account for these animals' lack of hair. (They say this about wild pigs too, despite the fact that most wild pigs also have plenty of hair.) The funny thing is that the different species of rhinos actually do show differing acclimation to water, and one species is actually a very good swimmer which spends a lot of time in swamps. To top it off, this species of rhino, the Sumatran rhino, also varies dramatically from the other rhinos in its degree of hairlessness. How so? I'll give you a hint: it's also commonly called the Hairy Rhinoceros.

That's right, rhinos are a clear refutation of the AAT/H notion that related species which differ in degree of acclimation to water will differ in hairlessness, with the more water-acclimated species being more hairless. What's more, the Sumatran rhino has more hair on its lower body, the part that's in the water more, which is the opposite of what AAT/H proponents say should happen when they look at humans.

Wild pigs often come in for the "they're aquatic cause they wallow" treatment from AAT/H proponents, which they consider important because AAT/H proponents often claim wild pigs are mostly hairless (they aren't). There *are* two types of wild pig which swim more readily than other types: African Bush Pig, or Red River Hog, an excellent swimmer (and extremely hairy), and the Babirusa, an excellent swimmer (and sparsely haired). Aquatic ape proponents often mention the sparsely haired Babirusa and its propensity for swimming yet never seem to mention the extremely hairy African Bush Pig and its similar propensity for swimming. For that matter, they only bother mentioning one subspecies of babirusa, the one most commonly found in zoos, *Babyrussa babyrussa celebensis*. Why not mention *Babyrussa babyrussa babyrussa* as well? Like the Sumatran rhino, perhaps one of the common names of *Babyrussa babyrussa babyrussa* gives us a clue: the Hairy babirusa (it's also sometimes called the Golden babirusa). Yes, subspecies of babirusas vary in amount of body hair but not in habitat or habits -- just like subspecies of mole rats

-- yet AAT/H proponents ignore those which don't support their idea. Using only one as an example, as AAT/H proponents do, is an example of their selective use of evidence, using only evidence which supports them and ignoring equally valid contradictory evidence.

Vernon Reynolds' Conclusions from the Valkenburg conference and *The Aquatic Ape: Fact or Fiction?*

AAT/H proponent Algis Kuliukas can pat himself on the back for getting me to do this page for my site. He has repeatedly, for quite some time now, insisted that I am remiss in mentioning Vernon Reynolds' summarising chapter for *The Aquatic Ape: Fact or Fiction?*, the volume which resulted from the 1987 Valkenburg conference which brought together AAT/H proponents and people with opposing views. After his latest (September 2009) I find myself inclined to agree with him; it's time.

Now there's one little difference here... okay, not so little. Algis has been insisting that Reynolds' *last paragraph* be mentioned, that it's basically an abstract and can stand in for the whole chapter. That's nonsense for several reasons. One is that it's not an abstract and shouldn't be used as one; another is that even if it were an abstract, abstracts vary a lot in how complete they are. Some do offer a virtual Reader's Digest condensed version of the paper, others are more like those teasers to that are designed to get you to watch the news: "What common activity may cause you to die tonight? Watch Channel 9 news at 11". So I'll mention Reynolds' final paragraph, but I'll also mention what Reynolds said about each of the claims made regarding the AAT/H at that conference. That only makes sense, to look at the whole of what he said (and what all four editors said in their Epilogue, for that matter, which I'll also do). Looking at only part would be as silly as looking at only the title of Alister Hardy's 1960 article and ignoring the body of the article, something Algis has also insisted should be done.

Here's the one paragraph Algis thinks should be mentioned:

CONCLUSION

A number of other arguments exist on either side, but I shall not discuss them all; in any case they are fully dealt with in the chapters of this book. All I have tried to do here is to pick out a number of key arguments, present them as fairly as possible, and then make a personal choice. Overall, it will be clear that I do not think it would be correct to designate our early hominid ancestors as 'aquatic'. But at the same time there does seem to be evidence that not only did they take to water from time to time but that the water (and by this I mean inland lakes and

rivers) was a habitat that provided enough extra food to count as an agency for selection. As a result, we humans today have the ability to learn to swim without too much difficulty, to dive, and to enjoy occasional recourse to the water.

So Reynolds is saying here that we have "the ability to learn to swim without too much difficulty, to dive, and to enjoy occasional recourse to the water". If that were indeed all there was to the AAT/H there would be no controversy whatever over it; no one doubts these things. It's also not much different from a number of non-human primates, so it's yet another of those areas where we resemble our primate relatives, except that many of them, for instance macaques, swim quite naturally without the need for being taught, so our ability to *learn to swim* as mentioned by Reynolds is actually worse than the ability of monkeys which are terrestrial. So that level of selection, the amount which led to our having those abilities, is a given. But that isn't all the AAT/H says, and Reynolds has some things to say about those other parts as well, in the parts of the summarising chapter outside that last paragraph. Reynolds is quite clear about what he thinks about them after reviewing the evidence given for them as argued by AAT/H proponents at the conference. In almost every instance he is clear in rejecting the AAT/H case; no wonder Algis wants the last paragraph to stand in for the rest.

This can be seen by taking the headings Reynolds used for each claim and his conclusion about that claim. I'll list these, along with a couple which need notes from me to point out errors:

Bipedalism

On balance, I would conclude that the arguments that bipedalism evolved on the savannah are stronger than those that it evolved in water.

Body hair

The arguments for seeing the one covering of hair on the human body as a savannah adaptation rather than an aquatic one seem convincing.

Sweat

In such circumstances sweating can be readily understood as a primary adaptation to a savannah environment rather than an aquatic one.

Subcutaneous fat

Overall, the arguments for an aquatic origin for human body fat seem weak or untenable, while the savannah arguments seem more convincing.

The 'diving response'

All in all, however, the evidence here points to the presence of an aquatic element in the environment, and one which was important enough to bring about some selection for agility in the water. There are things here that the Savannah Theory cannot properly explain.

My note: Reynolds bases this on a major error, seen in this sentence from this section: "It is not known whether this occurs in other primates, but the evidence suggests that it does not." This not true, the reflex and its innervation is the same in non-human primates. In fact, it's shared by all mammals, as well as diving birds, but present in diving mammals to a far greater extent than in humans and other primates. But since it's found in all mammals no matter what their habits, it's virtually certain to be an ancient reflex, a holdover from an older period of evolution. Since it developed at a time before any hominids existed, indeed before any primates existed -- its development cannot be expected to be explained by any hypothesis which is specifically about human evolution. The only conclusion we can draw from it is that, since we don't exhibit this reflex to the degree seen in diving mammals we don't show signs of having a lot of diving in our past. This is of course the exact opposite of the common AAT/H claim Reynolds is addressing here.

Marsh-wading

And the swimming and cooling adaptations that we see indicate a greater degree of adaptability of the hominids than a marshland habitat alone would produce, so that marshes may well have been a contributing habitat, and one that we should remember, but are unlikely to be more than this.

Location

But in the absence of any fossil pongid-hominid evidence from the Danakil area it has no real substance, and further evidence would be needed before it could be taken seriously.

My note: The above refers to Danakil Island idea, but doesn't get into the fact that there is no evidence that such an island actually existed, and the information talking about the Danakil alps during the period of sea level rise in question says it was a peninsula, with basalt floods connecting it to the rest of Africa. Even LaLumiere, whose work Reynolds is referring to here, knows and mentions this, but he apparently mistook this to mean a solid layer of rock many tens of miles wide (making an ecological barrier), while in reality basalt floods are commonly covered with vegetation. For instance the Columbia River Plateau area in northern Oregon is composed of basalt floods, as is the Massif Central in France. Reynolds also points out that there is no fossil evidence whatever supporting this claim. (By the way, in this chapter Reynolds accurately refers to LaLumiere as an acoustic physicist as opposed to Morgan's later inaccurate claim that he was a geologist.)

Clearly Reynolds came down hard on the AAT/H claims, rejecting virtually all of them. One he suggested had some merit only because he made the mistake of not looking critically at some AAT/H "data" -- about the diving reflex -- which was in fact false, and this also takes away the evidence he referred to when he said in his last paragraph that "there does seem to be evidence that not only did they take to water from time to time". This means there's actually less support than he suggests for even the limited amount of water use he sees likely. And what he accepts is merely that humans show "some agility in water" and may have used habitat near water at times, and occasionally utilised water resources, which is just like many non-human primates and of course has long been accepted by all paleoanthropologists. The AAT/H is much more than that, and the parts that are more than that are all rejected by Reynolds.

The four authors (Machteld Roede, Jan Wind, John M. Patrick, and Vernon Reynolds again) of the short Epilogue also come down against the AAT/H, which they refer to as a "somewhat far-fetched idea", despite the fact that two of the four authors were pro-AAT/H at the conference:

"Our general conclusion is that, while there are a number of arguments favouring the AAT, they are not sufficiently convincing to counteract the arguments against it."

Why AAT/H proponents would ever think Reynolds' conclusions were supportive of their idea is a mystery; it seems as if they simply don't read the whole thing, just those parts that fit their world view. Or perhaps they have a sort of "Morton's Demon" which keeps them from seeing things right before their eyes.

A Note about the term "Morton's Demon"

Over the many years I've been examining the aquatic ape idea I've often been asked whether I think the proponents are serious, whether they are honest, whether they are... well, you name it. I've tended to try not to state any firm conclusion because to do so definitively requires me to read minds, which I cannot do. But it does seem that many of them, perhaps not all but many, make mistakes and are resistant to data in ways that can only be explained by a sort of Morton's Demon. This is a term originated by Glenn Morton, a former young earth creationist (YEC) who, after working in the field of geology in the oil industry, found he couldn't hold his YEC views in the face of evidence. But he wondered that he did so for so long, even though evidence which proved his views false was all around him. He came up with the concept of the Morton's Demon, inspired by a famous thought experiment by the brilliant 19th century physicist James Maxwell, the Maxwell's Demon. There's a link to Glenn's short essay at the bottom of the page and in the references; let me post a few apropos excerpts here:

Maxwell suggested a famous demon which could violate the laws of thermodynamics. The demon, sitting between two rooms, controls a gate between the two rooms. When the demon sees a speedy molecule coming his way (from room A), he opens the gate and lets the speedy molecule leave the room and when he sees a slow molecule coming at the gate (from room A), he holds it closed. Oppositely, when he sees a speedy molecule coming at the gate from room B he closes the gate but when he sees a slow molecule from room B coming toward the gate he opens it. In this way, the demon segregates the fast moving molecules into one room from the slow ones in the other. Since temperature of a gas is related to the velocity of the molecules, the demon would increase the temperature of room B and cool room A without any expenditure of energy. And since a temperature difference can be used to create useful work, the demon would create a perpetual motion machine.

Maxwell's demon was shown to fail by Szilard who showed that the demon needed to use light (and expend energy) to determine a fast molecule from a slow one. This energy spent to collect information meant that the demon couldn't violate the 2nd law.

When I was a YEC, I had a demon that did similar things for me that Maxwell's demon did for thermodynamics. Morton's demon was a demon who sat at the gate of my sensory input apparatus and if and when he saw supportive evidence coming in, he opened the gate. But if he saw contradictory data coming in, he closed the gate. In this way, the demon allowed me to believe that I was right and to avoid any nasty contradictory data. Fortunately, I eventually realized that the demon was there and began to open the gate when he wasn't looking.

However, my conversations have made me aware that each YEC is a victim of my demon. Morton's demon makes it possible for a person to have his own set of private facts which others are not privy to, allowing the YEC to construct a theory which is perfectly supported by the facts which the demon lets through the gate. And since these are the only facts known to the victim, he feels in his heart that he has explained everything. Indeed, the demon makes people feel morally superior and more knowledgeable than others.

The demon makes its victim feel very comfortable as there is no contradictory data in view. The demon is better than a set of rose colored glasses. The demon's victim does not understand why everyone else doesn't fall down and accept the victim's views. After all, the world is thought to be as the victim sees it and the demon doesn't let through the gate the knowledge that others don't see the same thing. Because of this, the victim assumes that everyone else is biased, or holding those views so that they can keep their job...

But one thing that those unaffected by this demon don't understand is that the victim is not lying about the data. The demon only lets his victim see what the demon wants him to see and thus the victim, whose sensory input is horribly askew, feels that he is totally honest about the data. The victim doesn't know that he is the host to an evil parasite and indeed many of their opponents don't know that as well since the demon is smart enough to be too small to be seen.

References

"Cold and Watery? Hot and Dusty? Our Ancestral Environment and Our Ancestors Themselves: An Overview", by Vernon Reynolds, pp. 331-341, in *The Aquatic Ape: Fact or Fiction?* edited by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds, 1991, Souvenir Press: London

"Epilogue", by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds, pp. ____, in *The Aquatic Ape: Fact or Fiction?* edited by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds, 1991, Souvenir Press: London

[Original Morton's Demon essay.](#)

General Problems with the AAT/H

The method of the Aquatic Ape Theory (AAT/H)

In this section I try to present, as simply as possible, the problems with the way the AAT/H is argued, broadly divided into basic problems with the use and misuse of evolutionary principles by AAT/H proponents, and the many logical fallacies that AAT/H accounts typically fall into. Later sections deal more with examples of specific errors.

Problems with the idea itself

The most basic problem with the AAT/H is that it uses extreme environmental determinism. The proponents do this apparently thinking they are using the quite legitimate evolutionary principle of convergent evolution, but they do so in a very mistaken manner which suggests they really don't understand it. I have a short explanation of [convergent evolution here](#). It seems as though they've picked up the term without understanding what it actually means; most specifically they shy away from the fact that convergent characters are similar in function as well as in structure. That is, convergent features are similar in structure because they are similar in *function* due to environment, not simply similar in structure due to environment. In doing so AAT/H proponents commit a logical fallacy, the fallacy of explanation called "limited depth". Stephen's Guide to the Logical Fallacies describes this: "Theories which do not appeal to an underlying cause, and instead simply appeal to membership in a category, commit the fallacy of limited depth." They also expect this similarity to happen without regard to relatedness, which means that -- bizarrely -- they ignore the central element in evolution: phylogeny, relatedness, and descent with modification.

This is very bad, frankly -- no regard to phylogeny, no regard to function, just an expectation that environment should, somehow, cause all creatures within that environment to solve physical problems in the same way. Holding this view requires

one to not just ignore basic evolutionary principles, but ignore the fact that animals in the same environments very often exhibit different adaptations to solve the same problem. Ignoring basic scientific principles is bad, certainly, but ignoring facts is arguably worse. And expecting this extreme environmental determinism, and particularly adaptation to a particular environment, is an especially bad idea when trying to explain the evolution of an environmental generalist, and humans are one of, if not *the*, supreme environmental generalists in the animal kingdom.

They make the mistake of supposing that even very distantly-related animals in a common environment *should* evolve the same mechanisms for dealing with any given problem, for instance, believing that if hominids had a land-based transition they would've have used the same form of thermoregulation as "the wild ass and the camel" (that example is from Elaine Morgan, in her 1990 book, *The Scars of Evolution*: "*On the supposition that man's ancestors moved out from the trees to open ground and needed sweat-cooling, they might be expected to have followed the example of the wild ass and the camel in adapting their apocrine glands for that purpose.*"). This idea -- suggesting that developing an adaptation is like catalog shopping -- is an odd and non-evolutionary idea, which not only ignores the role of phylogeny in evolution (the core idea in evolution, actually) but also ignores the fact that animals in any given environment often develop many different mechanisms to deal with common problems. This often leaves AAT/H proponents in the odd position of being surprised that humans most closely resemble our primate relatives (for instance in sweat-cooling via eccrine glands rather than apocrine glands) instead of distantly related ungulates.

The AAT/H method also includes an ironic double standard: for instance, while the observation that non-human savannah mammals don't predominantly walk bipedally and predominantly cool by sweating via eccrine glands is considered devastating evidence against a terrestrial divergence for our ancestors, but the fact that no non-human aquatic mammals do so is simply ignored. And asking why humans are so *incredibly different* from aquatic mammals in the *actual traits which are ubiquitous in such mammals* is also apparently a no-no. So while expecting a fantasyland version of environmental determinism, it doesn't accept convergent evolution due to water in demonstrated, actual aquatic features -- just non-demonstrated, or not-really-the-same superficially similar features, or completely "false fact" features.

In every version of the AAT/H at least since Morgan, environmental determinism is paramount. This is the assumption that evolutionary changes are virtually always driven primarily by environment. Evolutionary change being driven by environment *can* happen but doesn't *necessarily* happen, *especially* with animals which are generalists, as hominids are -- in fact, humans are perhaps the supreme generalists

of the animal kingdom. Over the past decade or so, nutrition specialists Michael Crawford and Stephen Cunnane have introduced what amounts to a subset of environmental determinism, dietary determinism. This use of determinism has led to AAT/H proponents using a badly flawed understanding of convergent evolution, which is a terrifically useful concept when used accurately but which begets nonsense when used inaccurately.

One example of this -- and a real roadblock for AAT/H proponents' understanding of hominid evolution -- is the way they mistakenly assume that environmental determinism is the normal method within paleoanthropology. Of course paleoanthropologists examine environment in human evolution, especially since one of the central questions in human evolution has always been how we came to be able to use so many different kinds of environments while our closest relatives, the apes, are stuck being able to use only a very few. But the mainstream theories of human evolution typically deal with social interaction and food-getting, not environment, as drivers of selection. It is true that many of these theories are interested in how our ancestors were able to make changes that allowed them to live in more and more open savannahs over the years, because these savannahs were getting more widespread and because we always have wanted to figure out how hominids went from being forest-dwelling animals to an animal that can live pretty well anywhere. We're by far the most widely ranging mammal with the possible exception of the rats and mice that have followed us around the world.

AAT/H proponents seem not to understand this, as can be seen in the way they refer to mainstream paleoanthropological ideas as "savanna theory" or "terrestrial theory" or "mosaic theory". Contrast those terms to the actual theories used for many decades, even the discredited "killer ape", or others like the "hunting hypothesis", the "seed-eating hypothesis", the "gathering hypothesis", the "food-sharing hypothesis", or even hypotheses that deal with individual features such as the "radiator hypothesis". Note that most of these theories deal heavily with food, food-getting and sharing, and social interaction, and only tangentially with environment. Environment is mentioned, of course, and there's a big interest in figuring out what environment(s) we evolved in, and also of course any theory concerning human evolution must look at how we, as a species, managed to deal with a wider and wider variety of environments. It's one of the key differences between us and apes -- we did so much better at it; they managed only a few different environments, and not all that different when you compare to what we managed even through the time of *Homo erectus*. But the basic ideas generally concern behavior rather than environmental determinism, important since rather obviously it doesn't make sense to look for an environmentally deterministic solution to how an animal became more and more unconstrained by their environment. So, whether mistakenly or deliberately, AAT/H proponents build a

strawman of normal paleoanthropological theory, and whenever normal paleoanthropological theorists mention environment, as they naturally do, AAT/H proponents typically assume they're using environmental determinism just as the AAT/H proponents themselves do.

So from its earliest iterations, the predominate method used in constructing the AAT/H consists of this environmental determinism with an extremely heavy reliance on this flawed understanding of the evolutionary concept of *convergent evolution* as a mechanism and an equally extreme adaptationist view, along with a "pick and choose" browse through what I think of as "The Big Book o' Adaptations" much like you see done way too often in the poorly done forms of sociobiology (cause it's easier than doing it right). In this "pick and choose from the Big Book" method, evolution is like catalog shopping and animals get to ignore basic evolutionary principles and simply grab any feature they like from any animal.

Indications that, in Morgan's case at least, this problem is due to a severe lack of understanding of the basic principles involved, are that in the last 10 years (after decades writing on the subject of AAT/H-style evolution) she mistook the concept of "homoplasy" -- which is the umbrella term for both convergent and parallel evolution -- for a new term even though it's over 150 years old; and that she has claimed that convergent evolution in arctic animals are convergent in a "single shared feature" (white coloration -- several posts in 1995-6 in the sci.anthropology.paleo newsgroup, including Aug 01, 1995, Jan 25, 1996, Feb 01, 1996, as well as in her 1997 book), when anyone who does the slightest study of convergent evolution should know that arctic animals -- which are indeed a classic case of convergent evolution -- typically also have very short ears, shorter legs, and fur or feathers which insulates the bottoms of their feet (all to conserve heat). She also, in her Jan. 25, 1996 post, suggested changing the established evolutionary term convergence to one of her own devising, which is a strange bit of business one usually sees only in pseudoscience crackpots -- I am not saying she is one, mind you, but picking up their habits is a very bad idea for someone who wants to be taken seriously in science, as she clearly wishes. (Perhaps even odder is that she said she wanted to change the term "convergent evolution" because it didn't lend itself to being an adjective, when convergent *is* an adjective -- and she's a graduate with a literature major from Oxford, one of the world's best universities. Talk show host Arsenio Hall used to have a segment in his monologue, "Things that make you go hmmm....").

Another argument common in AAT/H circles provides a good example of how a melange of errors can be involved in creating one false argument. This is the question of "hairlessness" or "relative hairlessness" and what it means in

evolutionary terms. The usual category set up of "hairless" or "relatively hairless" mammals suggested by them contains humans, naked mole rats, and aquatic mammals. Because of their use of radical environmental determinism, this, they say, means that "hairlessness" or "relative hairlessness" is somehow due to either "aquaticness", "semi-aquaticness" or subterranean living (the naked mole rat), and since humans obviously are not rodentlike burrowers, and since no one argues they ever were, they must have been "aquatic" or "semi-aquatic". (Sometimes they include "wallowers" in the above categories, which adds the ridiculous implication that wallowing in mud is in some degree "aquatic" -- it gets worse than that; I've seen suggestions that damp ground or even humidity constitute an "aquatic" environment -- let's be kind and ignore those ideas).

For many people who don't know a lot of relatively arcane facts about many different mammals (which is most people, after all) this can sound convincing, but it's actually a number of "false facts" along with several logical fallacies (the second part of this page deals with some of the many logical fallacies that AAT/H proponents trip over regularly). First of all, the vast majority of aquatic mammals are neither "hairless" or even "relatively hairless" -- in fact this is only true of a very few fully aquatic mammals: whales and sirenians (manatees and dugongs) and one very large pinniped, the walrus. For the majority of these aquatic mammals the reason for their hairlessness (or relative hairlessness in the case of the walrus) is rather clearly the same as the widely accepted and well supported reason for relative hairlessness in elephants, rhinoceros, and hippopotamus -- it's their size and proportions.

All animals have to dissipate heat, and this is easier for animals which have more surface area for their volume. This is just like a radiator; if you look at one you see it is designed to have a great deal of surface area for its volume (if you look inside your computer you'll find this principle in use in heat sinks on heat producing components -- it's a general law of physics). An animal like an antelope has relatively long legs and neck, and therefore a lot of surface area for its volume compared to an elephant; the elephant therefore has more trouble getting rid of heat. The elephant has little hair for this reason (and tends to habits that don't build up heat as readily too) and as further proof that this is the reason, one can look at prehistoric elephants (and rhinos) which lived in much colder regions -- they were covered with hair, because they didn't have the heat load that their tropical cousins have to deal with.

The very large aquatic mammals have an even more difficult problem with surface area, because they don't have as long limbs as the very large terrestrial mammals. Now although most cetaceans are very large, there are some smaller versions (dolphins and porpoises) but besides some effect of volume vs. surface area there

are two other likely reasons for hairlessness in their case. One is that they are very swift swimmers, with highly specialized shapes for high speed swimming. The other may be simple phylogeny -- it may be that they are hairless because they had larger ancestors (early whales) who were hairless, and the modern dolphins and porpoises had to deal with this as best they could, just as every animal is to some degree a "victim" of their ancestry.

Then there's that naked mole rat; does it give some support for the idea that hairlessness is somehow, in some unspecified manner, due to environmental determinism as the AAT/H view would have it? Well, you can test that idea by wondering if they're simply an unusual curiosity among burrowing mammals. You could look at gophers, ground squirrels, and moles, or any number of burrowing mammals (all quite hairy except for the naked mole rat) but you don't even need to look that far afield, because there are 5 closely related species of mole rats, all with similar burrowing habits and habitat -- and the naked mole rat is the only one which is hairless. So what we see is that relative hairlessness is not environmentally deterministic as the AAT/H insists, not related to habitat but is instead an unusual feature with a few scattered species exhibiting this odd characteristic for idiosyncratic, species-specific reasons -- with the exception of those very large mammals with thermoregulation problems, a well known and well established solution to a problem of heat and body size vs. surface area.

But the AAT/H tries to explain this feature instead by first incorrectly describing it, then ignoring alternative explanations which make sense, then finally inaccurately shoehorning all these different beasties into a category and claiming the category makes the reason. This is a number of errors followed by the use of a classic logical fallacy (mentioned above and described further below), the fallacy of explanation called "limited depth".

Other problems with the AAT/H are that the supposed degree of aquaticism is rarely explained or defined and is never enough to account for the features mentioned -- since the features mentioned are typically those of seals, whales, and sirenians -- and that it doesn't account for continued existence of extreme deleterious features which should have been selected against. I mention these in more depth as part of the next section.

Problems with the presentation and argumentation of the AAT/H:

Logical fallacies

ad hoc ZING!ability:

AAT/H proponents (most at least) use an awfully "fluid" set of claims about aquaticness -- what I've called ZING!ability -- to go wherever and be whatever the proponents say it should be at any given time, dodging this way and that in internally inconsistent ways so they can answer any critics in an ad hoc fashion. Critics talking about swimming -- ZING! -- it's a wading and shorewalking creature... talking about walking along the shore and not swimming -- ZING! -- it's a creature capable of long distance open sea swimming. You can see this in the past on the subject of sweat and other matters -- the AAT/H has always had this trademarked ZING!ability, the ability to ZING! in and out of watery environs as much as they need to dodge criticisms. So when any difficulty arises with one of their claims, or a critic appears -- ZING! -- off the creature goes to the shallows or the shore, and the critic is accused of malfeasance.

This is connected with the last item in this list, the Fallacy of Ambiguity.

Strawman Argument

Definition, from the Talk.Origins Jargon FAQ: *"Strawman Argument: Stating a misrepresented version of an opponent's argument for the purpose of having an easier target to knock down."*

This is -- judging from its continual and widespread usage -- another essential AAT/H technique. An example is the assertion, echoed by almost all AAT/H versions, that paleoanthropologists claim that early hominids lived on "the arid savannah", which AAT/H proponents typically -- and inaccurately -- characterize as a waterless, treeless plain. The AAT/H is then said to be the only sensible theory, since early hominids were probably not well adapted for surviving on a treeless, waterless arid grassland. But paleoanthropologists don't make this claim; they *actually* refer -- quite clearly -- to an early hominid habitat consisting of mixed vegetation: trees, bushes, and grass (and some say the earliest hominid habitats were likely denser forests). Although online, and in her latest (1997) AAT/H book, Elaine Morgan has derided this correct version as some new idea -- a sort of last ditch desperate act by weaselly scientists to dupe their students and try to keep hominid evolution on land -- this is the [definition of savannah](#) which has been used for about a century.

Promoting their idea by tearing down another (strawman) idea:

This is a combination of the Strawman and False Dilemma logical fallacies, and is a mainstay of pseudo and fringe science of all stripes.

This is a very unscientific method; even if you could tear down another idea, it doesn't make any competing idea correct, and doesn't even bolster the competing

idea. Let me illustrate by assuming that some incorrect idea is generally accepted -- let's say the idea that $2+2=7$. Advocates of a competing idea say that $2+2$ actually equals 9 -- even if they successfully show that $2+2$ does not equal 7, it still won't mean that $2+2=9$, and it won't even make it more likely that $2+2=9$. Only showing that their idea is correct can prove their idea. I know it seems like this should be obvious, but for some reason it often isn't, especially when the competing ideas are not so obviously shown to be correct or incorrect as in basic addition.

And this would be a problem even if they accurately described the mainstream idea instead of using a strawman, and even if they accurately showed that mainstream idea to be wrong -- in fact they do neither, and that makes this an even bigger error on their part.

I think this one would be Irrelevant Conclusion (ignoratio elenchi):

Definition, from Stephen's Guide to the Logical Fallacies: *An argument which purports to prove one thing instead proves a different conclusion.*

This one is just plain saying that an appropriate expert said the opposite of what they actually said. Morgan has done this a number of times, one is her use of Derek Denton's work on salt. Citing Denton as her source, she said that in humans, innate physiological reactions to salt had been lost, and that this was a "specifically human characteristic -- the fact that we have no instinctive awareness of the state of the sodium balance in our bodies", while Denton actually said the exact opposite, that "in the human case, the cultural influences are acting in the context of an innate propensity", and that this was an "overall innate organization dedicated to salt ingestion along with the elements determining appetite response to sodium deficiency and to the hormones of the reproductive process."

Special Pleading:

This is holding another idea to a very different standard of evidence than your own, and is also a mainstay of pseudo and fringe science of all stripes. There are many instances of this shown on my site, but perhaps the classic example in the AAT/H is the proponents' common use of a double standard as they argue that the idea of hominids evolving bipedality in a terrestrial setting is badly damaged by the fact that no non-human terrestrial mammal is predominantly bipedal, yet the fact that no aquatic mammal is bipedal is brushed off as irrelevant.

A variation of this fallacy is using the ad hoc "not enough time" excuse for features or problems they don't want to explain, but plenty of time for bizarrely different features to appear, and sometimes disappear later without a trace:

actual ubiquitous aquatic features = "not enough time"

wading bird style long legs = "enough time"

shorter legs as seen in actual mammalian semi-aquatic adaptation = "not enough time"

"straight pelvis" = "enough time"

narrow pelvis as seen in actual semi-aquatic adaptation = "not enough time"

non-mammalian-style salt excretion system = "enough time"

kidney changes to deal with salt as seen in actual mammalian aquatic and semi-aquatic adaptation = "not enough time"

Fallacies of explanation: Limited Depth: Definition: *"Theories explain phenomena by appealing to some underlying cause or phenomena. Theories which do not appeal to an underlying cause, and instead simply appeal to membership in a category, commit the fallacy of limited depth."*

This is seen in the common AAT/H use -- or rather misuse -- of convergence and those characteristics they claim we have in common with other, generally unspecified, animals. Since they don't want to admit those animals are highly specialized aquatic animals which have been aquatic for tens of millions of years, they use phrases like "aquatics" and "where do we see" [these characteristics]. This is also an example of the fallacy called Coincidental Correlation, and is generally linked, in their work, to the next Fallacy of Definition.

Fallacies of definition: Too Narrow:

As seen, for instance, in the AAT/H Leaflet, they use any aquatic or semi-aquatic environment, yet try to restrict their critics to using one and only one specific terrestrial environment -- a strawman version of the savanna which includes only a limited number of actual savanna environments, and of course an even more limited range when you consider all terrestrial environments.

Appeal to Pity (argumentum ad misericordiam):

This is a bizarre (to my mind) and rather sad method, most often seen used by AAT/H proponents when talking about critics of Hardy or Morgan, although Morgan has used it in referring to herself. The critic is asked to think of the age, or sometimes the grandparent status, of the AAT/H proponent and to therefore not be so critical. Mistakes are excused by referring to the proponents' ages and/or supposed enfeebleness.

Ad hominem:

The most widespread use of ad hominem by AAT/H proponents is the careful creation -- particularly seen in Morgan's writings and career -- of the myth of the closed-minded academic establishment closing ranks against the embattled AAT/H

proponents merely because they are outsiders, not because the proponents make poorly thought out and poorly supported arguments. The Attenborough BBC radio show I critique on this site used this method rather heavily, taking up something like 15 or 20 minutes of the first half hour segment with this technique.

"Argumentum ad Assertion Repetitio ad Nauseam":

Although used to some extent by other AAT/H principles, this is Marc Verhaegen's baby, perhaps his most common online method (other than simple abuse directed toward his critics).

Appeal to Authority (argumentum ad verecundiam):

First, there are authorities and authorities, so to speak. For an authority to be properly used, that authority needs to have some validity as an expert on the subject at hand, and even then it's better -- far better -- to use someone's study or data rather than just quote them as an authority. It also helps, to say the least, if that authority actually says what you claim they say. For instance, if you were thinking of using Phillip Tobias as support for the AAT/H (as has been done, fairly frequently), you might want to reconsider since he has stated that he does not support the AAT/H.

Anonymous Authorities:

An example here would be Morgan's use of Daniel Dennett's anonymous "distinguished biologists, evolutionary theorists, paleo-anthropologists, and other experts", or her 24 Apr 1995 sci.anthropology.paleo newsgroup posting "I could easily cite between a dozen and a score of cordial, even blushmaking, responses from reputable journals and academics. I won't do it on the Net." In online communications this is generally chuckled at as the "lurkers support me in email" defense.

Hasty Generalization:

Seen, for instance, in Hardy's original seal comparison, variations of which have been made ever since by most if not all AAT/H proponents. Here a superficially similar appearance seems to show a similarity in traits, but only if you don't think at all about the massive differences in that feature between, in this case, seals and humans.

Fallacy of Exclusion:

Lots and lots of this; it is one of the most common methods used in arguing for the AAT/H. One example (of several on this site) are Morgan's mentioning anecdotal accounts of emotional tears in aquatic animals while burying such accounts of non-aquatic animals, even though both appeared in the same source (same book, same chapter) she used as a reference.

This can be done inadvertently, but is sometimes done deliberately as one-sided "lawyer-style" data mining, and sometimes you will even find pseudo and fringe science advocates admitting doing this without apparently thinking it's wrong. So ingrained can this habit become that it's possible for an advocate -- when seeing others *not* do this -- to think that the person *not* doing it can only be doing so "by mistake", as Algis Kuliukas did in my case regarding the conflicting data I found on hair and swimming speed.

For that matter, why, after making hair loss one of the cornerstones of the AAT/H for decades, supposedly as a convergent evolutionary feature due to water use, did none of the proponents bother to look for data on it -- why did it fall to me to do so? You just look it up at the library -- a couple of hours, tops -- why do they so often want others to do their research for them?

The AAT/H has a long tradition of ignoring the problem of predators, very unlike traditional paleoanthropology -- this is another example of the Fallacy of Exclusion.

Another common use of the Fallacy of Exclusion in the AAT/H is that it doesn't account for continued existence of extreme deleterious features which should have been selected against; these include (but are not limited to) ear exostoses (which causes ear infections and can rob us of hearing, and which aren't limited to people who swim and dive but which AAT/H proponents such as Marc Verhaegen claim are); diving problems such as heart arrhythmias and blackouts -- these and other problems are well known and represented in medical literature about diving (in fact there is a branch of sports medicine which deals with the health problems resulting from swimming and diving); and food allergies to things like shellfish, which are some of the most common and severe allergies found in people.

How about this: GULP! why do we still -- mind you after supposedly millions of years of our population swimming and diving -- why on earth do we tend to inhale when we go under, with obviously unfortunate results. And how when we run out of air, we automatically fight to breath, even when we're underwater -- obviously an incredibly dangerous thing to do. Actual animals which evolved diving abilities (as most all the AAT/H accounts claim for us) have physical adaptations which eliminate the impulse to inhale underwater -- we, even after supposedly millions of years of our population swimming and diving, don't. They got these adaptations through convergent evolution, the kind of evolutionary change, in the kind of environment, that AAT/H proponents claims for us.

and related:

Why, even after supposedly millions of years of our entire population regularly swimming and diving, do we drown so readily?

Oddly enough, some of these deleterious conditions, such as ear exostoses and dangerous food reactions, have been presented as evidence *for* the AAT/H by some of the proponents, namely Verhaegen and Morgan. This is actually another logical fallacy, called **Irrelevant Conclusion (*ignoratio elenchi*)**: Definition from Stephen's Guide to the Logical Fallacies: *An argument which purports to prove one thing instead proves a different conclusion.*

One other excluded item from the core of the AAT/H is taphonomy, the study of the distribution and numbers of fossils based on the rate of fossilization. This is a well established and understood principle and has been for many decades; one of the most common consequences is that animals and plants which live in water and along shorelines get fossilized at far higher rates than other organisms. This is where the AAT/H claims our ancestors lived: Why don't we therefore have lots and lots of hominid fossils? Why don't we therefore have many many more than we do?

Fallacies of Ambiguity:

The best example of this would be the AAT/H's definition, or lack thereof, of the degree of aquaticness they're talking about. The fact that this changes to suit any counter-argument makes it even more ambiguous. For instance:

The Aquatic Ape Hypothesis (AAH):

The hypothesis that water has acted as an agent of selection in the evolution of humans more than it has in the evolution of our ape cousins and that, as a result, many of the major physical differences between humans and the other apes may be explained, at least in part, as adaptations to moving (wading, swimming and/or diving) better through various aquatic media.

Elaine Morgan endorsed this definition earlier this year and I propose that this is what people use when discussing it from now on. If we do that, perhaps the next 44 years might be a little bit more productive in resolving this issue than the last.

Algis Kuliukas, 24 Aug 2004 sci.anthropology.paleo

In presenting this definition at several points during the early half of 2004, Algis made the following remarkable claim:

"To be fair to Hardy and Morgan they weren't really even at the stage to be able to define the hypothesis."

Algis Kuliukas, 24 Aug 2004 sci.anthropology.paleo

So here Algis claims (incorrectly, in my view) that even after decades of working on the idea, publishing account after account of the idea, and insisting that academics accept their idea, none of the idea's proponents had gotten to the stage of being able to define their idea!

If this were indeed true, it would be an incredible admission of incompetence, but in fact I think you can see pretty clearly in both Hardy's and Morgan's work what their idea was.

Besides the incompetence issue this claim raises, the two major problems you can see in this "mild" definition are:

1. It's so vague as to be meaningless.
2. Many of the major characteristics they claim arose in humans are found only in seals, whales, and sirenians, highly specialized aquatic mammals which have been aquatic for tens of millions of years, and not in any other aquatic and semi-aquatic animal, which indicates that it takes a long time and a lot of intensive, extremely specialized adaptation to an aquatic environment to develop these characteristics. Therefore, while it's certainly true that in order for a mammal to evolve these characteristics would take "more" water use than we see in apes, the "more" would be far more than any of the proponents want to accept; far "more" than anyone, even among the idea's presently (2006) living proponents, can see as even remotely possible. So much "more" that it makes the definition effectively meaningless.

What we see is really not so much an attempt to define the idea as to "define away" any possible objections -- this is not an honest approach to science. Here's the biggest problem I see with that attempt (which has been done, in various forms, by other proponents, including Morgan). The claim that we became the way AAT/H proponents say we did (and they're incorrect on most of the specifics they give as examples) through just a bit of wading and swimming certainly seems more mild, and the way it's often worded as an opener (for instance, "that water played some role in our evolution" or "a sea-side lifestyle") is often so generally nonspecific as to be both meaningless and non-controversial. And even if they simply said that wading may have been one of the many factors involved in our ancestors' bipedality that too would meet with little or no objection. But that's not all there is to it, once you "dive in".

First, they just can't seem to let it be one of many parts -- it's got to be *the* main thing, *the* key factor, and in doing so they continually discount or downplay the many other things that we see bipedalism is good for, and is used for by

present-day apes and other primates. They also vastly overstate the use of bipedalism when apes are in the water -- from their descriptions, you'd think it was virtually all the time, but it's not, and most of the time the bipedalism in the water is used during activities much like when it's used on land (for instance, during feeding on things which would otherwise be out of reach, or during displays). They sometimes also make the claim that bipedalism couldn't have arisen without the support of water, which has two major problems: one is that we know apes (and other primates) use it as part of their locomotor activities, some more than others; and there's the question, which never seems to get answered, of how wading in shallow water supports the weight of your body. And there's a contradiction if the AAT/H proponent claims (as they sometimes have) that we needed that water to support body weight for bipedalism, because we also see adaptations toward weightbearing in bipedality in hominids.

So this definition ends up being most notable for its vagueness, merely suggesting "more" water use than apes as a definition. Contrast that with the explanation set forth in the "Methods" section of a 2001 paper by Bininda-Emonds et al. ("Flippers versus feet: comparative trends in aquatic and non-aquatic carnivores", *Journal of Animal Ecology*, 2001, 70:386-400): "We consider aquatic carnivores to be those species in which the aquatic habitat inevitably plays a key role in the life-cycle of an individual..." which they then compare to definitions as given by others as they discuss the strengths and limitations of their definition. And note that this more specific explanation is in a non-controversial academic paper rather than what is supposed to be a groundbreaking new idea. With an ultra-vague definition like the AAT/H one offered, note that you could make a similarly vague definition, using virtually *anything* instead of "water" and make a case for it.

The AAT/H definition suggested is not only so vague as to be virtually meaningless, it's notable that this is the first attempt since Hardy at such an explicit definition, and it's taken over 40 years to show up (while a normal paper in a typical journal, as seen above, warrants such a definition as an obvious first step). However, there has always been an implicit "how aquatic" definition in the AAT/H, ascertained by the characteristics the proponents have used to build their case. It isn't the definition Kuliukas offers, and it's not one AAT/H proponents like to mention, and they are generally mighty coy about mentioning just what animals they're saying we resemble (preferring to use a vague category instead), but nevertheless the implicit definition is there. The next section will get into that.

ZING!ability, the shifting target

Versions of the AAT/H have been developed, primarily by Marc Verhaegen and more recently by Algis Kuliukas, to attempt to counter this problem by declaring that the period of change due to some degree of aquaticism is essentially the whole of human evolution. These versions have some of these same problems -- for instance claiming that various features are shared with humans and aquatic animals even those features are not similar -- and introduce or accentuate others, such as the problem of describing how a claimed small amount of extra water (or just *waterside*) use by hominids provides selection pressure for convergence with highly aquatic mammals such as seals, whales, and sirenia (manatees and dugongs).

It is very true that they sometimes say one shouldn't compare humans to those animals, and that our ancestors were not even remotely as aquatic as any of those mammals, but the features they say we share are found only in those mammals, so they are actually comparing us with them, and since they use convergent evolution to explain these supposed similarities, that comparison also necessarily implies that our ancestors were that aquatic. Naturally, this is not what they say explicitly, but look at their comparisons.

And that's the central problem with the stated definitions of "how aquatic" offered by AAT/H proponents versus the implicit definition that can be seen by their constant comparison to features seen only in seals, whales, and sirenia -- there's an enormous disconnect. The two cannot be reconciled, which should -- but doesn't -- give them pause.

This is part of AAT/H theorists going in for a classic technique among marginal theories, the shifting target. This is specifically seen in the fact that the "aquatic apes" have become less and less aquatic over the years, from being fully acclimated to sea life, diving, etc., to seashore-dwelling waders, to denizens of the shores of streams and inland lakes. (This is the essence of the versions put forth by Verhaegen and Kuliukas, for instance, and Morgan has also gone along with this method.) In explaining (excusing?) this shifting target strategy, AAT/H proponents simply say their theory is adjusting to new evidence, as any scientific theory should. However, actions speak louder than words, and when we examine AAT/H proponents' actions, what we see are attempts to have their cake even after they've swallowed it.

As just one example, with the change in the AAT/H mentioned above (i.e., a less aquatic wading ape), the AAT/H claim that these hominids would not and could not evolve bipedal posture and locomotion without being in neck-deep water falls by the wayside... and yet they still use that claim. (Morgan has insisted that she has never made the claim that water was necessary for support despite having had her own works quoted with sections that said just that. There's more on this in the later section on "Aldosterone, water, and bipedalism".)

And although you'll rarely see an AAT/H proponent state just what mammals they're talking about when they use (supposed but not accurate) similarities between humans and "aquatics", the characteristics they use show that the animals they *are* talking about are those highly or completely aquatic creatures, the seals, whales, and sirenians. But if it was unlikely that the original degree of "aquaticness" as suggested by Hardy would produce those changes in humans (and it was incredibly unlikely), to suggest these changes would happen with a much lesser degree of aquaticness is simply bizarre.

In essence, the AAT/H proponents want to have an aquatic mammal that isn't very aquatic, yet is forced to develop features seen only in fully aquatic mammals which have been aquatic for tens of millions of years. And even then, many of their claimed features are inaccurately stated; the features they mention are not actually like those of aquatic mammals. And even more telling, the aquatic features actually seen over and over in aquatic mammals due to convergent evolution -- which you would naturally expect to find in a mammal with an aquatic background -- are not seen in humans.

Sexual selection and the AAT/H

Sexual selection is interesting vis a vis the AAT/H because a surprising number of the traits that AAT/H proponents say are aquatic traits due to convergent evolution in response to the environment (and therefore selected via natural selection) are actually, and rather obviously, due to [sexual selection](#). This is obvious because, typical of sexually selected features, they differ between the sexes and appear at puberty instead of at the age the animal would start using the aquatic environment.

We see this, for instance, in the lifespan history of fat in seals versus humans. Seals rapidly gain fat while very young, and at a very early age they are essentially like their parents in their fat distribution and quantity. We, on the other hand, start off fairly fat as babies, drop within a few years to the leanest condition of our lives as children, and then rapidly build up fat at puberty, with radical differences in quantity and distribution of fat between boys and girls, and to top it off, at middle age our fat distribution changes once again. Also, in humans fat distribution and amount is far different in males and females, while in seals the sexes are very similar. The same huge differences between aquatic mammals and humans that we see in fat we also see with our hair and sebaceous glands. AAT/H proponents say they are aquatic traits, which means they must be due to convergent evolution selected via natural selection, but they are actually obviously sexually selected traits.

Perhaps this is why AAT/H proponents like Morgan have made a habit, both in books and in online writings, of downplaying the role of sexual selection in the emergence of these traits. She's done this in several books, including her most recent, the 1997 *The Aquatic Ape Hypothesis*, where she spends some time in Chapter 7 poo-pooing it, along with a misuse of a quote from Darwin that's unfortunately reminiscent of the method that creationists often use with Darwin's words. I cover this in the section "A brief critique...".

Perhaps the oddest problem with AAT/H proponents' methods

The AAT/H is typically presented as a series of two-part claims, i.e.:

1. Feature A cannot have evolved on land.
2. Feature A is found only in humans and aquatic animals.

Therefore Feature A is evidence of an aquatic transition from LCA to hominid.

Many claims have been made in support of the AAT/H using this general form. However, the quality of the research that has gone into these claims has been poor. Many of these features are not unique to humans amongst terrestrial animals. Many of them are shared by only a few aquatic mammals. And those features which actually *are ubiquitous amongst aquatic mammals* -- and which therefore would be expected to be found in a mammal which evolved in an aquatic environment -- are (just by coincidence?) features which AAT/H proponents have consistently ignored, because *none* of them are seen in humans or our hominid ancestors.

So one essential technique of the AAT/H is, ironically, to ignore all actual aquatic features.

Convergent Evolution and the AAT/H

The Concept of Convergence in Evolution

The AAT/H relies heavily on an important and quite valid evolutionary principle: the principle of convergent evolution. That said, it must also be recognized that the principle of convergent evolution as used in the AAT/H ignores a vital element of that principle. Therefore an explanation of this principle is in order:

From *Vertebrate Paleontology and Evolution* 1988, by Robert L. Carroll (W.H. Freeman and Company: New York) pg. 7:

"characters that are similar in structure and function but have arisen separately rather than from a common ancestor are termed **convergent**."

And he uses G.G. Simpson's description of the terms "convergence" and the similar term "parallelism":

"Convergence is the development of similar characters separately in two or more lineages without a common ancestry pertinent to the similarity. Parallelism is the development of similar characters separately in two or more lineages of common ancestry on the basis of, or channeled by, characteristics of that ancestry."

Convergence in evolution (unrelated living things sharing similar traits which they don't share with a common ancestor) is due to a similarity in function in these traits. These are often due, as the AAT/H suggests, to these living things (we'll just consider animals here) living in similar environments which are different from their close relatives. There are classic examples which are often cited, for instance arctic mammals and birds, which often have white coats of fur or feathers, and in the case of mammals, smaller ears to conserve heat. (Conversely, similar animals which live in extremely hot areas often have large ears to radiate heat -- note the difference between arctic foxes and fennec foxes in ear size.) Another environment which creates convergent features is an aquatic one, but unfortunately for the AAT/H, humans share none (!) of the aquatic traits actually due to convergent evolution (take a look at the list in the first 5 "Relevant Questions for the Aquatic Ape Theory").

The essential part of the principle which the AAT/H shies from is that these convergent characters are similar in *function* as well as in *structure*. Their similarity in function is what creates selection pressure for similarity in form, so examining and explaining this similarity in function is critical to the *appropriate* application of the principle of convergent evolution. Perhaps many of the oft-repeated errors by AAT/H proponents arise because they typically present examples of alleged similarity in structure without any examination of function. (Of course when they also present features which *aren't* similar they compound their errors.)

Why do they make these errors about convergent evolution? I'm not entirely sure, but part of it seems to be a simple lack of understanding of basic evolutionary

concepts. It stands to reason that if you're putting together a theory about human evolution, you would learn the basic concepts of evolution (and physiology and other related studies). It's not easy, certainly, but it's not *that* hard. And it's necessary.

Definition of savannah

From 1986 *The Savannahs: Biogeography and Geobotany*, by Monica M. Cole (Department of Geography, Royal Holloway and Bedford New College, University of London). Academic Press: London.

pg. 4:

"An understanding of the problems that have beset the formulation of an agreed definition of savannas requires consideration of the origin, historical and present use of the word."

pp. 4-5:

"The term savanna is believed to originate from an Amerindian word which, in a work on the Indies published in 1535, was used by Oviedo y Valdes to describe 'land which is without trees but with much grass either tall or short'. Subsequently Grisebach (1872), Drude (1890) and Schimper (1903) extended its use to include grasslands with trees and thereafter the term was used to describe the mixed trees and grass types of vegetation found in all tropical latitudes."

pg. 5:

In central and southern Africa savanna is used for open deciduous woodlands, including those locally known as miombo, that are composed of fairly tall, mesophyllous trees and a well defined grass stratum, for parklike vegetation comprising grasslands studded with microphyllous trees of low to medium height, for grasslands with scattered clumps of trees or bushes, for treeless grasslands of tall perennial mesophytic grasses and of short annual grasses mixed with perennial grasses with narrow rolled leaves, and for open forms of vegetation composed of scattered low growing microphyllous trees and shrubs and a ground layer of perennial and annual grasses. The term bushveld is used locally for the parklike forms of savanna which together with the low tree and shrub forms are regarded as the most typical savannas."

The Concepts of Natural Selection and Sexual Selection in Evolution

Darwin wasn't the first to come up with the idea that evolution took place; many others had thought that was the case, but had no reasonable explanation for how it could happen. Darwin's amazing achievement was in coming up with the

mechanism by which an unconscious nature could cause these changes to happen in ways that seemed, in hindsight, to be on a path toward a goal. (Creationists still struggle to understand this despite Darwin's clear writing and painstakingly thorough scholarship.) He came up with two methods of selection -- natural selection and sexual selection. Some people think that sexual selection should be considered a subset of natural selection, but there are distinct differences between them. The difference which I think is important is that the apparent (in hindsight) promotion of traits through natural selection is actually just natural selection not killing them off (either directly or through "genetic death, ie. the lineage dying out). Natural selection doesn't cause a search for better or more optimal ways of doing things, it just lops off those that really don't work well at a given time, even if those changes could be useful at some future time. Natural selection, in essence, only works "against" the organism; everything "for" it is accidental, although the end result, in hindsight, looks like there was a direction.

Sexual selection, on the other hand, is generally selection "for" a feature or features, actively chosen by another organism (though not necessarily consciously so). It can also be active competition, usually between males, for instance either fighting or displaying, although there is often an element of choice by the non-fighting sex (usually females).

One of the strongest hints that a given feature is likely to be due to sexual selection is the timing of their appearance during the lifespan -- these traits typically aren't seen until the animal is able to mate.

The AAT/H characteristics list

If you've surfed the web for AAT/H info, or read a news story about it, you've seen the info from the comparative list in the AAT/H leaflet. This was apparently first written up to be given out at talks by Elaine Morgan, and her grandson posted it on his site soon after. I assume it was written by Elaine Morgan. I see that AAT/H proponent Algis Kuliukas has disputed the origin of this leaflet ("The authority of this leaflet has to be questioned from the start"); Morgan's grandson referred to it as "the leaflet printed by Accent of Swansea for Elaine Morgan". (Perhaps Kuliukas assumes Morgan's grandson was lying, or perhaps he assumes Morgan's printer (Accent ADC of Swansea) just made up all those claims on their own.)

My site deals with these characteristics on the "AAT/H claims and the facts" page, but since this handout has been and continues to be the basis for much AAT/H discussion, I've put the original list here along with an annotated copy, which lists the actual facts behind the leaflet's false "facts".

The AAT/H leaflet list

Characteristics	Humans	Apes	Savannah	Aquatics
Habitual Bipedalism	Yes			
Loss of body hair	Yes		Yes	Yes
Skin-bonded fat deposits	Yes			Yes
Ventro-ventral copulation	Yes	Yes		Yes
Dimunition of apocrine glands	Yes			Yes
Hymen	Yes			Yes
Enlarged sebaceous glands	Yes			Yes
Psychic tears	Yes			Yes

Loss of vibrissae	Yes			Yes
Volitional breath control	Yes			Yes
Eccrine thermoregulation	Yes			Yes
Descended larynx	Yes			Yes
The Aquatic phase took place more than 5 million years ago. Since then, <i>Homo</i> has had five million years to re-adapt to terrestrial life. It is not surprising that the traces of aquatic adaptation have become partially obliterated and have gone unrecognized for so long. But the traces are still there as the table indicates. The "Yes" in column 3 refers to the bonobo; in column 4 the rhinoceros and the elephant.				

That's a pretty impressive list, judging from people's responses to it. And since it is true that paleoanthropological theories don't explain why this list's correlations between humans and "aquatics" exist, it's no wonder uninformed people think the AAT/H is at least as valid as those from more traditional sources. The problem comes when people become informed by other than AAT/H proponents. It turns out the "similarities" listed in the leaflet's list are bogus, so explaining why they exist is impossible -- they aren't real. However, as Darwin long ago noticed about false "facts", they keep showing up, again and again.

Notice in the headings of this list the use of the word "Savannah" instead of terrestrial or land-based; while the next heading is simply "Aquatics". The AAT/H proponent feels free to use any *purported* feature from *any* aquatic animal (no matter what their size, habitat, lifestyle, or length of time being aquatic) as evidence for their theory, while attempting to restrict the opposing theory to only a small subset of terrestrial mammals. This is the Logical Fallacy called "Special Pleading", which is a common AAT/H technique. Even doing this, they continually have to look to *supposed* similarities with aquatic mammals which have highly specialized aquatic features which they've developed over the course of tens of millions of years of intensive aquatic living. We're talking seals, sirenians (dugongs and manatees), and whales here; they are the mammals the list refers to in each of its categories (although even then the similarities aren't real). Seals have been aquatic for about 25 million years or so; sirenians and whales for perhaps 50; all are so specialized they are incapable of living a natural life which isn't aquatic (especially sirenians and whales).

Here's the list again, with a reality check added. For more info on these features, look through this site; there's a lot here and I'll be adding more info to the site as time goes on.

The AAT/H leaflet list...annotated

Does the AAT/H leaflet tell the real story? The right-hand column shows when the AAT/H claims are true, false, or simply wildly misleading.

Characteristics	Humans	Apes	Savannah	Aquatics	Reality
-----------------	--------	------	----------	----------	---------

Habitual Bipedalism	Yes				True: Although AAT/H proponents claim that an aquatic lifestyle forces an adaptation to bipedalism, even they have to admit there is no evidence for this claim. They also leave out the many mammals which do use <i>occasional bipedalism</i> , perhaps because it's common in many terrestrial mammals. The <i>only</i> mammals which typically use occasional bipedalism for all the reasons humans do -- locomotion, feeding, sentry and display behavior -- are terrestrial mammals, such as primates and kangaroos; no aquatic mammal uses bipedalism.
---------------------	-----	--	--	--	--

Loss of body hair	Yes		Yes (rhinoceros and elephant)	Yes	Highly misleading: All the mammals mentioned have hair and skin characteristics radically different from humans'. And among aquatic mammals only cetaceans, sirenians, and the semi-aquatic hippopotamus; all are either very large sized and/or high-speed swimmers with highly specialized skin and which have been very specialized aquatic mammals for several tens of millions of years.
-------------------	-----	--	--	-----	--

Skin-bonded fat deposits	Yes			Yes	Wrong: Our fat deposits are like other primates and very unlike fatty aquatic mammals, both in pattern and life history.
Ventro-ventral copulation	Yes	Yes (bonobo)		Yes	Wrong: Also seen in oranges, black-handed spider monkeys, and occasionally in woolly spider monkeys and gorillas; and only some very specialized aquatic mammals which have been aquatic for several tens of millions of years.

Dimunition of apocrine glands	Yes			Yes	Wrong: Humans follow progression seen in great apes; and in aquatic mammals only in whales, very specialized aquatic mammals with highly specialized skin and which have been aquatic for several tens of millions of years.
Hymen	Yes			Yes	Wrong: Found in various terrestrial mammals, including lemurs, llamas, guinea pigs, hyenas, elephants, rats, horses, and some species of galago, as well as in aquatic mammals such as toothed whales, seals, and sirenia.

Enlarged sebaceous glands	Yes			Yes	Wrong: Only lemurs have sebaceous glands as large and numerous as humans'. The AAT/H claim apparently originally referred to a study which only compared the sebaceous glands of phocid (earless) seals to otarid (or earred) seals.
Psychic tears	Yes			Yes	Wrong: Proven in humans only; claimed in several aquatic <i>and</i> terrestrial mammals.

Loss of vibrissae	Yes			Yes	Wrong: Humans follow progression of fewer vibrissae (sensory whiskers) seen in great apes; and in aquatic mammals seen only in <i>some</i> whales, very specialized aquatic mammals which have been aquatic for several tens of millions of years.
Volitional breath control	Yes			Yes	Wrong: Humans have greater control than other mammals but breath-holding is seen in other mammals, including many primates and in dogs.
Eccrine thermoregulation	Yes			Yes	Wrong: Humans and some primates only; not seen in <i>any</i>

					aquatic mammal.
Descended larynx	Yes			Yes	Wrong: Humans' larynx very different from aquatic mammals, both in structure and life history, and descended larynx found in various terrestrial animals, including chimps and various deer species.

Relevant Questions for the Aquatic Ape Theory

The AAT/H relies heavily on the principle of convergent evolution. According to the AAT/H, convergent evolution produced massive changes to our body structure (ie. bipedalism) and systems in very little time -- producing, among other things, a salt excretion system like those seen in crocodiles and sea birds, rather than the one used by marine mammals.

In fact, the AAT/H is so dependent on the principle of convergent evolution that it uses it to make statements which ignore the crucial role of phylogeny (relatedness) in evolution. For instance, the AAT/H says we did not evolve on land because some of our adaptations, such as our method of thermoregulation, are different from the methods used by other distantly-related-to-us mammals in that habitat, such as "the wild ass and the camel". This ignores the fact that an organism can't just evolve whatever is handy, picking it from some sort of evolution catalog of desirable features, and it ignores the fact that in any given environment, the creatures who face common problems often evolve different solutions to the problem.

Convergent or parallel evolution does operate, though, and can be demonstrated when you see different organisms evolving common solutions to shared problems.

This is common in aquatic species; however -- ironically -- the AAT/H consistently ignores the many *actual* examples of convergent evolution in action in aquatic and/or marine animals.

Therefore, these questions -- the first 5 are about traits which actually *are* aquatic and/or marine characteristics and which are ubiquitous and due to convergent or parallel evolution -- come to mind.

These are five *real* aquatic traits, traits shared by virtually all aquatic mammals, even those which are not closely related. Why don't humans exhibit these *actual* aquatic adaptations?

1. Why don't humans have really small ears (or no external ears) like virtually all aquatic mammals?

2. Aquatic mammals have shorter legs, or no legs, relative to land-based animals, including their land-based relatives. Early hominids had legs similar in length to our relatives. According to the AAT/H, there was enormous selection pressures that produced massive changes to our skeletons for an aquatic life, and according to the AAT/H, this was due to convergent evolution. Why were our legs, unlike those of other aquatic animals, exempt from convergent evolution?

3. Humans have young that are born less developed than our relatives, and they develop more slowly as well. Aquatic mammals have young that are quite advanced compared to similar terrestrial mammals, including their terrestrial relatives (for example, seals as opposed to land-based carnivores), or which grow very quickly, or both. Why did the purported aquatic hominids change in the *opposite* direction from other aquatic mammals?

4. All marine mammals produce milk that is extremely rich in fat and protein, and very low in lactose (milk sugar). It ranges from a low of 20-25% fat for sea otters to 30-60% for pinnipeds and whales; protein ranges from 5-15% or more; lactose is virtually non-existent. By contrast, human milk and cow's milk are about 2-4% fat and 1-3% protein; lactose in the milk of terrestrial mammals is typically 3-5%. The lactose content of human milk is as high as 6-8 percent. Why are humans so *unlike* all marine mammals and so *like* terrestrial mammals?

5. One AAT/H claim is that we evolved in saltwater and therefore adapted in the same manner as aquatic animals, with convergent evolution supposedly evolving a salt excretion system like that seen in sea birds and crocodiles (and many terrestrial reptiles and birds). Why didn't we adapt as all marine mammals have done, via a change to our pre-adapted kidneys -- which are the regulated salt excretion system used by *all* mammals -- developing better hormonal control over rate of urine formation and concentration via the kidneys, as has repeatedly happened, due to convergent and/or parallel evolution, in cetaceans, pinnipeds, and sea otters?

A common answer I've gotten to these questions -- in fact the only answer I've gotten to these questions -- is that "there wasn't time". However, according to the AAT/H, there was enough time to cause massive changes in our physiology, some in ways not seen in any other mammal. There was supposedly enough time to change not only our pelvis, but also to change our salt excretion system into systems that resemble those in sea birds and reptiles (but completely unlike *any* mammal, terrestrial or aquatic), to change our sebaceous glands into seal-like features, and other such incredible leaps. Yet somehow there just wasn't quite enough time to cause those very changes we see in virtually all aquatic mammals?

This AAT/H response is special pleading of the first order.

Some additional relevant questions for AAT/H proponents

6. Some AAT/H proponents say our pattern of hair orientation is due to water flow while swimming. This requires that a great deal of time be spent swimming with the crown of our heads forward and our arms held along our sides, pointing toward our feet. In her latest book, Elaine Morgan also suggests we simply held our heads out of the water while doing this, making for an even *more* improbable swimming position. Some questions:

- A. How did we swim in this position?
- B. How did we breathe while swimming in this position?
- C. Why didn't we look where we were going?
- D. The reduction of hair and its orientation has been said to be an adaptation for speed in the water; how did we swim *fast* in this position?
- E. The reduction of hair and its orientation has been said to be an adaptation for speed in the water to escape sharks (Hardy 1977, reprinted 1982); but while large

land-based predators run approximately 3-4 times as fast as humans, sharks swim approximately 3-6 times as fast as the *fastest Olympic swimmers*. Why were we able to swim away from sharks but not run away from land-based predators?

7. We know that our close relatives, chimpanzees, survive in spite of large terrestrial predators in open savannah woodland (and in fact apparently do so more ably than their forest cousins), and we have observations of them in the wild that show us how they do so. How did the postulated aquatic transitional population deal with common aquatic predators such as crocodiles and sharks?

8. Hardy and others say that during this supposed aquatic phase of the transition, we learned to make sharpened stone tools, knives, and even spears, and to hunt and butcher large animals: why did we quit doing these extremely useful things for the next 4-6 million years after supposedly learning to do so?

9. Elaine Morgan claims that the evidence of vitamin A poisoning seen in the *Homo erectus* specimen KNM-ER-1808 was from eating fish -- rather than from eating carnivore liver, the generally accepted likely cause (carnivore liver stores vitamin A in often-toxic levels) -- and that this shows we had been doing so since the transition from apes (i.e., for 4-6 million years). If so, why hadn't we either:

A) developed a resistance to such toxic reactions to a food which supposedly had been eaten regularly for approximately 4-6 million years before that time?

B) learned, in that 4-6 million years, how to avoid toxic poisoning from a supposedly common food?

C) if we had developed such a resistance and continued those habits, why did we lose this adaptation?

10. Marc Verhaegen makes a similar claim that a pathological condition (which we might reasonably expect to see weeded out of an aquatic population) is actually evidence for some degree of aquaticness. These are exostoses in the ear canal, bony growths that he says only occur after a great deal of time (he usually says years) either swimming or diving. They are seen in a small number of Neanderthal and *Homo erectus* fossils. He calls them "undeniable indications of frequent diving" (sometimes he says swimming) and says that they must have been wading and diving because "Their ear exostoses leave no other choice..." (ellipses in original --

links to these posts are in the references). But ear canal exostoses actually can be due to almost any irritation, most commonly either cold air or cold water. They are not due only to cold water, as he repeatedly claims, and this is widely known and readily available in medical texts (he is a medical doctor). Even online medical sources mention this:

"Exostoses occur when the external canal is repeatedly exposed to cold air or water."

"[Geriatric hearing loss](#): Understanding the causes and providing appropriate treatment" by Michelle C. Marcincuk, MD and Peter S. Roland, MD

"Exostoses are bone growths that often develop when the ear canal is repeatedly exposed to cold water or cold air."

[Yale New Haven Health Library](#)

"The exciting cause of ear exostoses, where the predisposition to these exists, may be anything mechanical or chemical that produces prolonged irritation, with consequent hyperaemia to inflammation of any part of the bony meatus."

Ales Hrdlicka quoted in [Dry Bones](#), Chapter 4."

The obvious question here is how can a medical doctor be ignorant of the medical literature on such a well-known ailment about which accurate information is so readily available?

Recap: Actual ubiquitous convergent features of aquatic mammals:

1. Smaller ears (or no external ears) than their non-aquatic relatives.
2. Shorter legs, or no legs, compared to their land-based relatives. (Early hominids had legs similar in length to our relatives.)
3. Aquatic mammals have young that are quite advanced compared to their terrestrial relatives (for example, seals as opposed to land-based carnivores), or which grow very quickly, or both. (Humans have young that are born less developed than our relatives, and they develop more slowly as well.)
4. All marine mammals produce milk that is extremely rich in fat and protein, and very low in lactose (milk sugar). It ranges from a low of 20-25% fat for sea otters to 30-60% for pinnipeds and whales; protein ranges from 5-15% or more; lactose is virtually non-existent. (By contrast, human milk and cow's milk are about 2-4% fat and 1-3% protein; lactose in the milk of terrestrial mammals is typically 3-5%. The lactose content of human milk is as high as 6-8 percent.)

5. Marine mammals have extremely large and/or extremely lobulated kidneys (due to convergent and/or parallel evolution).

AAT/H claims and the facts:

Below are some key AAT/H claims juxtaposed with the relevant, and contrary, facts. Most also have either references or links to more information on these facts or in-depth examinations of the claims (more will be added as I complete them):

Claim: Human hairlessness is explained by an aquatic past

Fact: Humans' relative [hairlessness](#) is unlike aquatic mammals, because A) most aquatic mammals aren't hairless; and B) those few that are have skin that's *radically* different from humans (there's a link for this in the seal skin and sweat section below).

Claim: The pattern of human hair alignment is strikingly different from apes and indicates streamlining for swimming.

Fact: The pattern of human hair alignment is only very slightly different from apes. Also, in order for this pattern to indicate streamlining for swimming we would have to be swimming with the crown of the head facing straight forward and your arms held at your sides. Just take a look. This is so easy to see, you've got to wonder how AAT/H proponents can make the claim, or why it's swallowed so uncritically. There's also the problem that humans are not even close to being fast swimmers to whom streamlining therefore might help (more info in the "hairlessness" link above).

Claim: The human body responds the same to the act of standing up as it does to surgery or massive haemorrhage but this reaction doesn't occur when standing up in water.

Fact: Ooh, this is a thorny thicket; neither part is true but are misrepresentations of facts twisted about to make a point which isn't true sound true. To see how and why, you'll have to read this link on [aldosterone and bipedalism](#).

Claim: Only humans and marine mammals shed salty tears.

Fact: All primates shed salty tears (see tears link below).

Claim: Only humans, Indian elephants, and aquatic mammals cry emotional tears.

Fact: Humans are the only mammals proven to cry emotional [tears](#). There are no animals other than humans which have been scientifically proven as having emotional tears. However, there are unproved accounts of many other mammals crying emotional tears, but these are not just aquatic animals; they include dogs and wolves, seal, sea otter, lab rats, cats, cows, pigs, lambs, horse, a kangaroo and a gorilla.

Claim: Only marine reptiles and birds have salt glands.

Fact: [Salt glands](#) are found in many non-marine reptiles and birds, including ostriches and other birds, and many lizards, including iguanas, chuckwallas, and others.

Claim: The human response to salt indicates we evolved in a salt-water environment.

Fact: Human [responses to salt](#) are similar to terrestrial mammals, including chimps. Mammals which live in salt-rich environments do not exhibit these responses as humans do. Our salt mechanisms indicate a terrestrial past with a large herbivorous component to our diet, unlike the AAT/H claims.

Claim: Human infants naturally swim while other non-aquatic mammals' infants can't.

Fact: The infant "[swimming response](#)" has been found in all mammals tested.

Claim: Only humans and aquatic animals exhibit the "diving reflex".

Fact: The "[diving reflex](#)" is found in all mammals.

Claim: Only humans and aquatic animals can hold their breath.

Fact: Non-human, non-aquatic animals can and do hold their breath (refs in diving reflex link above).

Claim: The descended larynx of humans is like that of aquatic mammals, and must have arisen in an aquatic environment. Although it's necessary to make all the complex sounds we use in speech, it cannot have arisen for that purpose, because it wouldn't be useful for that purpose in its initial stages.

Fact: The descended larynx of humans is not particularly similar to those which are found in (only a very few) aquatic mammals (refs and info in diving reflex link above). Previously here I'd said that the evidence from the fossil record also indicates that this feature developed several million years after the purported aquatic period, but research on the larynx over the past shows this is actually a far more common trait than previously thought, and is primarily because of vocalization (see the "[descended larynx](#)" section of the breathholding, descended larynx, and diving reflex link).

Claim: Non-human primates have nostrils that point forward, unlike humans.

Fact: What can I say; Old World primates are in fact called *Catarrhine* primates precisely because their nostrils face down. Morgan likes to try to have this one both ways; while she claims that forward-facing nostrils are detrimental to aquaticism, and that we had a human-like nose several million years before the bones on our ancestors' faces indicate they did, she takes the nose of both male proboscis monkeys (with its downward-facing nostrils) *and* of female and juvenile proboscis monkeys (which face as much forward as other Old World monkeys) as aquatic adaptations. (Scientists who study these monkeys' behavior say it's sexual selection, as is true of all sexually dimorphic traits which aren't due to differences in use.) She even has a drawing of a juvenile proboscis monkey swimming in her latest book which, according to her theory, should have water shoved up his forward-facing nostrils. Why, if it's no problem for a monkey, would it be such a big problem for hominids as to force a massive change? Morgan doesn't see the contradiction.

Claim: Our ancestors wouldn't have changed from quadrupedalism to bipedalism, because initially bipedalism would be less efficient than quadrupedalism.

Fact: Actual tests of chimpanzees by Taylor and Rowntree in 1973 (*Science* 176: 186-187) has shown that bipedalism is no less efficient for them than quadrupedalism. It wouldn't be for our ancestors, even if they evolved from knuckle-walking apes such as chimps. Also, the consensus over the last few decades has been that the LCA was far more likely to have been a brachiating (swinging from branches) ape rather than a knuckle-walker, which makes it even less of a problem to be bipedal. In fact, brachiating apes -- such as gibbons -- *virtually always walk bipedally* when they are on the ground.

Claim: Proboscis monkeys use bipedalism more often than other primates and often walk bipedally as "merely an alternative locomotor mode of getting from A to B."

Fact: Morgan bases this claim on several seconds of film taken by Japanese filmmakers, which showed several proboscis monkeys walking bipedally. On this subject, I just (August 9, '01) watched a TV program, "The Secret World of the Proboscis Monkeys", and over the course of the hour, those obnoxious primates simply refused to do any bipedal walking. Perhaps it was because it was French filmmakers this time, or maybe the anthropological conspiracy quashed all the bipedal episodes. Or, just possibly, it's what years of observations by primatologists tell us: Proboscis monkeys don't walk bipedally more often than other primates (all primates use bipedalism occasionally).

Claim: It was too dangerous for our ancestors to live on land during the transition from ape ancestor to hominid. The water provided safety from predators.

Fact: The water environment would be far more dangerous than the land environment; the [predators](#) there are more numerous and harder to deal with.

Claim: Our ancestors couldn't have dealt with predators on land, because the only way to do so is to run away, and we weren't fast enough and there were no trees to climb.

Fact: Not only were there trees in the hominids' environment (see [savannah definition](#) if you haven't already), but it is unlikely we would have been limited to

running from predators. How we probably would have handled them is how chimpanzees handle predators now (see the predators link just above).

Claim: The body temperature of normal, healthy humans is the same as that of whales, rather than our primate relatives or other terrestrial mammals, and it doesn't fluctuate, while that of terrestrial mammals does.

Fact: The [body temperature](#) of normal, healthy humans is like that of our primate relatives, it does normally fluctuate, and it's not like that of whales.

Claim: Hymens are an aquatic trait.

Fact: Besides humans, hymens are found in lemurs (fellow primates, you'll note), guinea pigs, mole rats, hyenas, horses, llamas, elephants, rats, horses, and some species of galago, as well as in aquatic mammals such as toothed whales, seals, and sirenia. Among these, toothed whales, seals, and sirenia are aquatic. And note that the aquatic ones are fully aquatic, not casual dip in the water types; once again AAT/H proponents are comparing us to mammals which have been fully aquatic for tens of millions of years.

There's another thing: the hymen seems to vary an awful lot, so much that it doesn't look like you can make the easy comparisons across species the AAT/H relies on. For instance, guinea pigs' hymens are quite different from most others, and those in whales are apparently different enough that various textbooks refer to them as "vaginal bands" or refer to "a hymen-equivalent possibly present in juveniles" ("Reproduction in Marine Mammals" by Ian L. Boyd, Christina Lockyer, and Helene D. Marsh, in *Biology of marine mammals* edited by John E. Reynolds III and Sentiel A. Rommel, 1999).

Also, AAT/H proponents suggest that the reason for a hymen in humans was to seal off the female reproductive organs from waterborne parasites and such; but since the hymen is generally absent from the time of first intercourse (and very often before) this protection wouldn't be available for much of the female's lifespan.

Claim: Vibrissae (sensory whiskers) are absent only in humans and in aquatic mammals.

Fact: Among aquatic mammals, vibrissae are actually absent only in some types of whales (whales such as blue, fin, and humpback whales have them) and of course they are abundant and very sensitive in most aquatic mammals. They are, however, also absent in other, terrestrial, mammals, such as tree shrews and the monotremes (platypus and echidna). The great apes have few vibrissae compared to other mammals, and their absence in humans seems to be yet another of the "continuation of a trend" features we see in primates (hair and sweat glands are other such features). A few minutes search on the web (using the term "vibrissae" with either "primates", "comparative", or "whales") easily turned up this information. Why do AAT/H proponents not do even so easy and basic research as this before making their claims?

Claim: Human fat quantity and distribution is like that of aquatic mammals; it is adapted for insulation and swimming in an aquatic environment. Humans have subcutaneous fat which is bonded to the skin rather than anchored within the body, unlike non-aquatic mammals.

Fact: Human [fat](#) characteristics show no sign of any aquatic adaptation, and are radically different from the aquatic mammals AAT/H proponents say we resemble. Human fat deposits are anchored to underlying depots, just as those of all mammals are. Human fat deposits are found in the same places, and are anchored the same way, as those of other primates.

Claim: Seals sweat through eccrine sweat glands, like humans, because aquatic mammals lose their apocrine sweat glands.

Fact: Seals don't sweat via eccrine sweat glands, and in fact the sweat glands of seals *are* apocrine glands (refs in "skin" link below).

Claim: Human sebaceous glands waterproof the skin, like the sebaceous glands of seals (and that "Sebum is an oily fluid whose only known function in mammals is waterproofing the hair and skin.").

Fact: Sebaceous glands cannot waterproof human skin (which is why your skin wrinkles when wet). This is because human skin is very different than the [skin](#) of seals. And that's not the only evidence that human sebaceous glands are not an aquatic adaptation. The primary function of sebum, the output of the sebaceous glands, is to produce scent (generally as a sexual attractor). This is true of a variety

of mammals, including humans. In some seals, sebum can also keep their highly specialized skin pliable as an aid in waterproofing (refs in "skin" link).

Claim: Aquatic mammals copulate facing each other, like humans do, while other terrestrial mammals don't.

Fact: This statement is at odds with the *facts* about [mating postures](#).

Claim: Hominids couldn't have developed large brains without Omega-3 fatty acids (DHA) from a shore-based diet.

Fact: Simply untrue, and rather obviously as well. See [brains and "brain food"](#).

Hair and "Hairlessness"

One of the AAT/H's principle claims to fame is that it explains the human hair pattern and amount of hair relative to apes; it's generally at or near the top of their list of "aquatic traits". The AAT/H claims that humans lost their body hair as a result of an aquatic or semi-aquatic phase in our past, claiming this is an aquatic adaptation. There are two fundamental flaws in this reasoning:

1. From the fact that many humans today have a lot of body hair, we can safely surmise that this feature varied during the time span of our species, *Homo sapiens sapiens*, instead of changing -- as the AAT/H typically requires -- during the transition from the LCA to hominid (LCA = **last common ancestor** between apes and hominids).
2. A great many, probably most, types of aquatic mammals are hairy, so we see that losing body hair is not an aquatic feature.

There's more on that second point on a later page which lists all the aquatic and semiaquatic mammals and how many times aquaticism and semiaquaticism has evolved in extant mammalian lineages. Not only are the majority of aquatic and semiaquatic mammals hairy, you should look at how often aquaticism or semiaquaticism arose in extant mammals -- how many distinct lines lead to an aquatic and semiaquatic mammal species today. For instance, beavers evolved their semiaquaticism separately from otters, but all pinnipeds (seals and walrus) today descend from a common aquatic ancestor. When you examine how many distinct times aquaticism and semiaquaticism arose in mammals which are around today, you find that it's 31 times, and that of that 31 times, only 3 times did it lead to hairless mammals. The vast majority of times it led to still-hairy mammals.

What's more, of those aquatic mammals which don't have body hair; almost all are very large mammals, just like the land mammals which have little body hair. The reason large mammals tend to not have body hair is that they overheat if they have it (large shapes have a ratio of less surface area to volume than small shapes, so they lose heat slower -- this is basic physics and has been shown to be the case for animals). Also, all of those aquatic mammals which don't have body hair are fully aquatic mammals incapable of living long out of water, and they've been fully aquatic for many tens of millions of years, far longer than hominids of any kind have existed. Contrary to what you'd expect from the AAT/H claim, there are *no* hairless aquatic and semiaquatic mammal except for those which are fully aquatic and have been for tens of millions of years, or are either very large and almost certainly hairless due to the physics of thermoregulation, or they are *both* very large and fully aquatic for tens of millions of years.

None. Not one.

[Note: At this spot on this page I previously made the incorrect statement here that phocid seals (also called eared seals or true seals) don't have body hair. That is not so; they are well-covered with stiff hair.] Seal skin is also very different from human skin in important ways that indicate we didn't have an aquatic background. The outer layer of seal skin is composed of "flattened, solid, keratinized cells" (King 1983:143, *Seals of the World*; British Museum (Natural History), Oxford University Press). "This may be contrasted with such a layer in man for example, where enzymatic digestion of the non-keratinous parts of the outer cells of the stratum corneum results in these cells becoming mere flakes continuously shed. Such a condition in a seal could lead to the skin becoming waterlogged" (King 1983:143, *ibid.*). The skin of cetaceans sloughs off at an incredibly fast rate compared to humans (every 2 hours for a dolphin compared to about 18 hours for humans) and is also waterproof.

Clearly human skin is very unlike that of hairless aquatic mammals. Unlike the skin of these aquatic mammals, human skin quickly becomes waterlogged (this is why your fingertips wrinkle when you're in water), an obviously undesirable trait for a purportedly aquatic animal.

Body hair and convergent evolution

The aquatic ape theory uses the evolutionary concept of convergence to explain why aquatic mammals have little or no body hair. Ignore for a moment the undeniable *fact* that aquatic mammals don't always (in fact, *don't usually*) have little or no body hair, and look at the principle of convergence as it relates to this

loss of body hair. How does hair or the lack thereof fit into the principle of convergence? Does the AAT/H explain this feature?

Assume for a moment that humans today are pretty much the same in terms of having little or no body hair -- okay, I know it isn't true, you know it isn't true, anyone who takes a moment to think or who has ever seen Robin Williams take his shirt off in a movie knows it isn't true, but for a moment, *pretend* it is true. Then we have several groups of mammals which have little or no body hair: extremely large mammals, extremely fast-swimming mammals with specialized skin, naked mole rats, some pigs, and humans. The group of extremely large mammals include elephants, hippos and rhinos, as well as whales, manatees and dugongs. Walruses, Northern sea lions (Steller sea lion), and elephant seals are often said to be in this group, but they do have hair over almost all of their bodies. The group of extremely fast-swimming mammals with specialized skin includes porpoises and orcas, and some of those aforementioned whales. Humans quite obviously don't fit into any of these categories, so right away we can see that the AAT/H seems doomed to fail to explain this supposed hairlessness of humans by claiming it's a convergent aquatic evolutionary trait. The question that AAT/H proponents typically fail to ask is: what about that necessary feature of evolutionary convergence, *similarity in function*?

Mostly AAT/H proponents are coy on this point, declining to point to any such similarity, as if mere contact with water. even just wading, is enough to create this change. Those that are more honest in their theorizing can come up with only one reason: to swim faster. Looking at the list above, we can see that it seems to be the only possible reason. But is it a sensible reason? Would selection pressure have worked its magic on this feature to make us faster swimmers during an aquatic phase?

Catching fast food and escaping aquatic predators would seem like a good idea to say the least, but would losing our body hair have done it? There are two parts to this, does it make us faster? and is "fast" fast? Second question first: human swimmers are, sadly, pathetically slow. The fastest human swimmers in the fastest Olympic event can't quite manage 6 miles per hour, and they're one heck of a lot faster than average people (twice as fast or faster). Given the long legs which propel them, we can be pretty sure that they're one heck of a lot faster than our early ancestors too. Yet 6 miles per hour is slow; it's a speed which can be easily exceeded on land by children (that fat kid down the block can do better than 6 mph). It's also deadly slow in the water, crocs and sharks are several times faster. A shark wouldn't even have to kick into high gear to catch an Olympic swimmer (assuming the swimmer saw the shark coming, which is a whole other ball of wax -- see the section on predators).

Now for the first question, does lack of body hair make us faster swimmers? Sure, we already know it can't make us *fast enough*, but does it make us faster at all? Since we've seen swimmers shave their body hair for years now, it might surprise you as it did me to find that there was no real attempt to experiment and see if it actually worked until fairly recently.

When I looked this up some time back, in literature on sports physiology, I found one study which showed an effect that could possibly help swimming speed, although actually helping swimming speed was not demonstrated. ("Influence of body hair removal on physiological responses during breaststroke swimming", Rick L. Sharp and David L. Costill, *Medicine and Science in Sports and Exercise* 1989: 21(5), pp. 576-580.) That study showed a difference in build-up of lactic acid in the muscles in athletes who had shaved their bodies, a slight increase in distance per swim stroke, and an "insignificant decline" in heart rate, which all together indicated reduced effort in swimming. This, it was assumed, would lead to an increase in swimming speed. They also showed a slight increase in coasting distance (compared to a control group) when a swimmer dove into a pool when that swimmer had shaved their body hair. These differences, they felt, could be as high as 3-4%, which would work out -- for the fastest Olympic swimmers in the fastest event -- to as much as one-quarter of a mile per hour. This would be important for competition swimmers, where an increase in a quarter mile per hour might easily mean the difference between defeat and victory against other human swimmers, but against swimmers -- like crocs and sharks -- that still swim 2-5 times faster, it would have a negligible effect.

A later study, by Krüger, Mikoleit, and Heck, showed a definite difference in swimming speeds before and after a full body shaving, but ironically does *not* support the idea that reducing body hair appreciably affects swimming speed in humans. This study was done with both males and females, and the study notes that while "results show that body shaving leads to a clear gain in performance. It can not be explained by the reduction of water resistance by the loss of hair, as the nearly body-hairless girls show the identical effect as the boys with clearly more body hair". The males actually averaged 1/4 of 1% more improvement, and the overall improvement hovered around 2.5%; this suggests that only 1/4 of 1% of the improvement was due to hair causing drag while the other 2 and 1/4% improvement was something else -- the test demonstrated a difference in lactic acid buildup in the muscles. The main result shown in this study -- the difference in lactic acid buildup in the muscles -- is the same difference shown in the first part of the Sharp and Costill study. Lactic acid building up as your muscles work is a good thing -- up to a point. It acts as an energy source for those muscles, but if too much builds up the muscles' ability to maintain a high workload decreases. This is why the reduction of lactic acid buildup is important for athletes like these

swimmers studied. So it may be that for whatever reason, shaving off all body hair, rather than simply having less body hair to start with, reduces lactic acid buildup. It may also be that shaving off all body hair leads to an improved feel for the water allowing the athlete to use better technique, which is supported by some studies but not by the Sharp and Costill study. Krüger mention that studies have shown that this effect from shaving off all body hair only works if it's done just 2 or 3 times per year; more and the effect is decreased. So whatever the reason this wouldn't help explain why hominids at some point started having less body hair, and it would only help them in water if they were shaving their body hair, all of it. That's certainly not any AAT/H scenario I've ever heard of.

The other experiment I know of related to hair and swimming speed showed a decrease in drag in seals when hair is present. In the book *Mammal Skin* (1982) Sokolov describes experiments which showed this effect: "Experiments on models covered with the skins of *Phoca vitulina* and *Pusa sibirica* show a reduction in drag as compared with rough, bare skin. Hence the damping role of pinniped pelage during swimming (Mordvinov and Kurbatov, 1972). A hydrodynamic tube experiment with a dummy having a Baikal sealskin pasted over it reveals that hair is conducive to an increase in the boundary layer thickness, to a reduction of the speed pulsation level within 1 to 10 khz, and to a reduction in drag." (Sokolov 1982: 60; *Mammal Skin* by V.E. Sokolov, University of California Press: Berkeley).

So is smooth better? The jury is still out. The past few years many more competition swimmers have been wearing body suits, but some say new records are not due to the suits. And if the newfound speed is due to these suits? The characteristics of these suits make them additional arguments *against* the AAT/H position on hairlessness, since they are ridged fabrics. The idea behind the suits is to recreate the same effect as seen in that experiment with that hairy sealskin and the skin of dolphins, which is covered with very small ridges. Of course the AAT/H wants it both ways; smooth bodies *and* hairy heads. At any rate, the speed isn't nearly enough; sharks and crocs would literally have an Olympic swimmer for lunch in a contest of swimming speed.

Let me make this point about the slowness of human swimming speeds very clear; here are the swimming world records as of July 2009.

Note: the long course is 50 meters and the short course is 25 meters so there are twice as many turns and pushoffs in the short course; the short course records are faster than the long course due to the pushoff being faster than any swimming stroke. Since we are examining swimming speed rather than pushoff I'm using the records for the long course for all distances.

Men's 50 meters freestyle	26 Apr 2009	5.32951289 mph; 8.59598854 kph
---------------------------	-------------	--------------------------------

Men's 100 meters freestyle	13 Aug 2008	4.74388948 mph; 7.65143464 kph
----------------------------	-------------	--------------------------------

Men's 200 meters freestyle	12 Aug 2008	4.33566434 mph; 6.99300699 kph
----------------------------	-------------	--------------------------------

Women's 50 meters freestyle	19 Apr 2009	4.65776294 mph; 7.51252087 kph
-----------------------------	-------------	--------------------------------

Women's 100 meters freestyle	27 June 2009	4.24657534 mph; 6.84931507 kph
------------------------------	--------------	--------------------------------

Historically (early events were generally 100 meters and longer):

Men's 100 meters freestyle	03 Dec 1905	3.39209726 mph; 5.47112462 kph
----------------------------	-------------	--------------------------------

Men's 200 meters freestyle	18 Aug 1902	3.00403769 mph; 4.84522207 kph
----------------------------	-------------	--------------------------------

Women's 100 meters freestyle	18 Oct 1908	2.34947368 mph; 3.78947368 kph
------------------------------	-------------	--------------------------------

Women's 100 meters freestyle	10 June 1912	2.76923077 mph; 4.46650124 kph
------------------------------	--------------	--------------------------------

Mind you, there are good reasons to believe these speeds are faster, probably much faster, than our earlier ancestors could manage; here's a few of those reasons:

1. these are highly trained swimmers, the best in the world
2. these are *Homo sapiens*, who have long arms and legs for powerful swimming strokes, compared to australopithecines
3. these swimmers are using a swimming stroke almost certainly unlike what would be used by our ancestors, who probably used a dogpaddle just as other primates do and for that matter just as most mammals do if they aren't specialised for swimming

But as you can see, even these elite athletes are very slow. Even in the fastest event in modern times the fastest men in the world can go slightly more than 5 mph, for less than half a minute (when I was in my 20s I could *walk* that fast for 4 hours or more at a time). The fastest women in the fastest event are not even up to 5 mph. When you look at the 100 meters, where the effect of the pushoff is lessened and swimming speed becomes more of the total, even the fastest men in the world can't manage 5 mph. And that's now; look at the older records: the men barely over 3 mph and the women not even getting to 3 mph; that's an average walking speed.

Even if you could make these speeds 10% faster, it would be so slow compared to potential predators that the predators would have no problem catching any swimmer they wished to. But the facts don't show a possible 10% gain in speed, at most they show 3-4% *if you remove all hair*, which didn't happen with humans. Couple that with the fact that aquatic predators are rarely seen before they attack and the notion that we could escape them by swimming a half mile an hour (or less) faster is just silly.

One AAT/H proponent, Algis Kuliukas, has taken my points above as evidence for an aquatic background. His mistakes in doing so are instructive.

First, he continued a long-standing AAT/H tradition of dishonestly reporting only *part* of the evidence I found; the part about the study that points to hair removal as possibly producing a slight benefit in speed. But he ignored the other two items of evidence I also found, both of which suggest the opposite effect. While the research I found showed conflicting, and therefore, inconclusive, data, he turned that into non-conflicting and conclusive data (and another AAT/H "false fact") by dishonestly reporting only that part of the data he'd seen which supported his position (and since he read it here in the above paragraphs, we can be sure he was dishonest in his omissions).

Second, he made an error in reasoning by asserting this speed increase would help individual, less-hairy hominids because they would swim faster and be at the front of a fleeing pack of which the rearmost individuals would be most at risk. There are several problems with this assumption. One is that what he describes is seen only in animals which form extremely large herds or schools (unlike any ape/hominid groups) and/or which are not incredibly slow compared to their potential predators (unlike us versus crocs or sharks). Another is that, as my section on aquatic predators explains in detail, these aquatic predators are rarely seen until they have *already struck* -- in the vast majority of cases, *fleeing never comes into the picture*.

Perhaps most instructive is what his reaction says about an all too typical AAT/H proponent's attitude toward data. I reported all the data I found on hair and swimming speed, both suggesting that hair helped and that suggesting no hair helped, and I did this because that's the way you do science -- it's the only honest and sensible way to do it -- but he assumed that I did so "by mistake" . That he would assume so tells us a lot, I'm afraid, about his own attitudes toward data, and what it tells us is not good, and certainly not reassuring vis a vis the reliability of AAT/H proponents' research.

He's also invented another piece of data which he's been using a lot online by taking one small bit of data out of one of the studies I mention above. I describe this on the page about him.

Summary

If we look around us, we know that not all humans have extremely long head hair, even when it is allowed to grow without being cut, and not all humans have little or no body hair. Yet both these characteristics are said to be explained by an aquatic

period in our ancient past, long before the species *Homo sapiens sapiens* arose. Here again is the problem with that view:

1. We know that humans today are one single species.
2. We know that humans today vary in body hairiness, and in length of head hair.
3. We know that these characteristics vary regionally.

Consequently, we know that this variation post-dates the transition to our species, so we know this sort of variation can and did arise within a few tens of thousands of years.

Therefore, since we see that either of these characteristics can come and/or go that quickly (a few 10s of thousands of years or less), any theory claiming it happened during the transition from common ancestor to hominid can only be an unwarranted assumption. Yet this is what the AAT/H states, and this unwarranted assumption is what AAT/H proponents consider a key piece of evidence for their view.

The change from the ancestral characteristics of body and head hair -- whatever those characteristics might have been -- may have happened first during the timespan of *Homo sapiens sapiens*, or it is possible that it changed and rechanged many times before that. But we do know that it can, and did, change quickly, and that it has changed during the timespan of our species.

So besides the intractable problem that changes in body hair would be insignificant in terms of swimming speed, the speculation in the AAT/H about how these hair characteristics changed has three basic problems:

1. In both cases, the AAT/H speculates a change into a characteristic which is not a characteristic of our species, just of some members of our species; and
2. since we know these characteristics changed during the span of our species, the AAT/H explanation that it happened millions of years before that must be incorrect; unless
3. it's treated as a sort of nonsense speculation, like speculating that we may have had stripes for a few million years but they disappeared without a trace.

Aldosterone, water, and bipedalism

This is, as I mentioned, a fascinatingly thorny little thicket involving some persistent "false facts" begun by Elaine Morgan in her 1990 book, *The Scars of Evolution*, and repeated by others since. At its heart is a central contention of the Aquatic Ape Theory: a major reason that these water environments were necessary for the evolution of bipedalism is to help support the body weight of our ancestors

while walking upright. Hardy made quite a point of this in his first article on the subject, and it has been a central tenet since. Morgan devoted an entire chapter to it in *The Scars of Evolution* (Chapter 3, "The Cost of Walking Erect", and part of chapter 4). It was there she brought up the aldosterone evidence...

Wait, just a sec... I can't keep typing without mentioning this. Sorry, but this one just slays me. Back when I was posting on this subject in the sci.anthropology.paleo newsgroup, Elaine made the most amazing claim. I *have* to mention it -- it's even apropos, since it sheds light on her research and debating style. Here's what she said about this on the 4th of July, 1995:

"I would like to reply to some serious misrepresentations.

"Jim Moore says that "all" (sic) supporters of AAT claim that a major reason for the evolution of bipedalism was that wading in water helped to support the body weight. I do not know of anybody that says this."

Frankly, I was astonished. I mean, she did get the theory from Hardy; she reprinted his articles on the subject in her 1982 book. Didn't she ever actually *read* his work? But more than that, what astonished me was that barely 5 years before she posted, she'd published an entire chapter and a half on just that point. Now, I've seen authors who were ignorant of writings that contradicted their position, and I've seen authors who were ignorant of writings that support their position, but I've never seen another writer who was so ignorant of what they themselves had written. Incredible.

1990 *The Scars of Evolution* Elaine Morgan. Souvenir Press: London

pg. 47:

In the aquatic scenario the position is reversed. Walking erect in flooded terrain was less an option than a necessity. The behavioural reward -- being able to walk and breathe at the same time -- was instantly available. And most of the disadvantages of bipedalism were cancelled out.

Erect posture imposes no strain on the spine under conditions of head-out immersion in water. There is no added weight on the lumbar vertebrae. The discs are not vertically compressed. (An astronaut in zero gravity gains an inch in height in space, and immersion in water is the nearest thing to zero gravity on planet Earth.)

pp. 47-48:

Water thus seems to be the only element in which bipedalism for the beginner may have been at the same time compulsory and relatively free of unwelcome physical consequences.

From these quotes you can see that the support provided by water is considered to be a critically important part of the purported aquatic transition to bipedalism. Despite Morgan's claim to the contrary.

Okay, I'm better now. Where was I? Oh yeah...

It was there (chapters 3 and 4 of *Scars*) she brought up the aldosterone evidence. Aldosterone is a hormone which regulates salt balance in the body (there's a lot of salt-related material in the AAT/H, and therefore on this site). Let's see what she has to say:

1990 *The Scars of Evolution* Elaine Morgan. Souvenir Press: London.

pp. 31-32:

"Endocrine glands sited above the kidneys produce hormones which respond to actual or potential physical 'emergencies'. The best known is adrenalin (epinephrine), the 'flight-or-fight' hormone. In situations arousing fear or anger it causes sugar to be released into the bloodstream supplying instant energy, plus clotting agents in case the situation leads to violence and bloodshed.

A hormone which responds to 'emergencies' of a different kind is aldosterone. Its function is to regulate blood pressure, and to inhibit the excretion of salt. The 'emergencies' which stimulate the production of aldosterone are listed in medical textbooks as:

- 1 surgery;
- 2 anxiety;
- 3 a diet deficient in salt;
- 4 haemorrhage;
- 5 standing up.

The first four items apply to all mammals and are readily understandable. If there is a deficiency of salt in the body tissues, aldosterone conserves salt by preventing its excretion into the urine. Loss of blood due to surgery or haemorrhage means that the blood volume must be restored; the action of aldosterone increases the fluid volume of the blood. As for acute anxiety, some of the physical manifestations (sweating, vomiting, loosening of the bowels) are notorious squanderers of salt reserves. An increase in aldosterone production will guard against all these contingencies, since this hormone can inhibit salt excretion in the sweat and faeces as well as in urine.

The fifth 'emergency' (standing up) applies particularly to bipeds. Rising from bed or from a chair produces a six-fold increase in the amount of aldosterone in the blood. This bears no relation to the amount of exertion needed to attain or sustain erect posture. Experiments have been conducted where a volunteer is strapped to a pivoting board which can be tilted on its axis, thus raising the body to the perpendicular with no physical effort involved. The production of aldosterone still rises just as steeply.

The explanation once again lies in the pull of gravity on the bloodstream. When we stand up, the blood tends to drain away from the head and the heart and pool in the lower limbs. It so happens that the main baroreceptors which monitor changes in blood pressure are situated in the neck. For a quadruped this is a perfectly appropriate site; blood pressure at this point will be fairly representative of the pressure throughout the body; when these baroreceptors register a change in blood pressure, they trigger appropriate responses such as increased output of aldosterone.

But the receptors are incapable of distinguishing between the 20 per cent drop in pressure which would result from a massive haemorrhage in a quadruped, and the 20 per cent drop in the head and neck region which signals in a biped the act of standing up. The aldosterone levels respond in the same manner to standing up as to surgery."

Now *that* sounds serious. Thankfully there's a solution at hand for those poor hominids:

pg. 47 (Ibid.):

"Standing up in water does not trigger secretion of aldosterone, salt retention or higher blood pressure. The reverse is the case: head-out

immersion causes a prompt and marked *fall* in systolic and diastolic blood pressure, plus increased excretion of salt in the urine."

Lucky little buggers. If only it were true...

The reality about the standing up "emergency"

As is common with Morgan research, it's hard to know where to start in pointing out errors; I think we'll start with her list, as "listed in medical textbooks". She left out a few key points. Before I get into particulars, maybe I should make a point clear. When I initially researched Morgan's statements in these chapters, I found she was wrong about many points, but I couldn't tell at that time whether she was wrong because she'd used references which were wrong or whether she had accurate references but misrepresented what they said. As a student of the history of science, I wanted to know which it was. So in December 95, in sci.anthropology.paleo, I asked her to provide those refs (she'd by that time said she was more than willing to do so), and eventually, she posted them. The refs were good, in fact, I had found them already in my own research on the subject. The list she got from the *Encyclopedia Britannica*, which I hadn't used but when I checked it out, it was the exact same list I'd found in William Ganong's 1993 *Review of Medical Physiology*. There are some interesting differences between the list in these books and the list in Morgan's book.

In her book, Morgan uses a rhetorical device which seems to be an attempt to get the reader to think that not just these aldosterone receptors, *but the total animal* would "respond in the same manner" to standing up as it would to massive hemorrhage or to surgery. But of course many other factors would enter in and create a massive difference between the two types of stimuli. Morgan, by carefully crafting the *false context* that these books consider all the items on this list to be "emergencies", fails to differentiation between several distinct types of causes of this aldosterone build-up. Both the textbook and encyclopedia article, however, point out that the *actual emergencies* (surgery, anxiety, physical trauma, and hemorrhage) as opposed to normal bodily functions (like standing up, which Morgan inaccurately lumps in with the emergencies) and slow-onset illnesses, also cause an increased excretion of powerful steroids called [glucocorticoids](#). They point out that these glucocorticoid levels are unaffected by high potassium intake, low sodium intake, constriction of interior vena cava in thorax, standing, and secondary hyperaldosteronism (in some cases of congestive heart failure, cirrhosis, and nephrosis).

While Morgan's list mixes together these normal life events (like standing up) and slow-onset diseases with actual emergencies and clearly implies that the sources call them all emergencies (as she does) the sources themselves don't do that. It was merely Morgan's invention.

The reality of water immersion

The second part of Morgan's claim rests on scientific experiments on the effects of water immersion therapies. She presents the results in the section on page 47 of *The Scars of Evolution*:

"Standing up in water does not trigger secretion of aldosterone, salt retention or higher blood pressure. The reverse is the case: head-out immersion causes a prompt and marked *fall* in systolic and diastolic blood pressure, plus increased excretion of salt in the urine."

With these statements, Morgan completes her argument that our hominid ancestors would be able to stand up in water without the temporary effects of blood pressure drop and rise in aldosterone. Of course the effects of these changes when standing up are actually pretty minor, as even Morgan admits ("It is a system which works smoothly and is in general benign." Ibid. pg. 33), but perhaps a case could be made that things might have been easier for our ancestors without it. Morgan tries to make that case, as seen above, and it would make a lot of sense... if it were true.

You see, these changes during water immersion don't happen in time to prevent these very temporary effects of standing up, as Morgan suggests. Her source, Murray Epstein (who actually is an experienced researcher on this subject) points out that the aldosterone drop during immersion Morgan is so happy to see isn't a quick one, as it would have to be for it to be a benefit in the aquatic ape scenario; instead it begins "as early as 60 min of immersion" (Epstein 1984. pg. 181). The increased excretion of salt Morgan mentions takes even longer to come about, several hours after the increased excretion of urine (which is why you can get dehydrated in water unless you can drink it; of course in the commonly postulated AAT/H saltwater environment you *can't* drink it). The increase in urine excretion itself doesn't happen for an hour or so after immersion either. Epstein also notes the blood pressure decrease isn't quite so dramatic as Morgan seems to imply ("water immersion is associated with a significant, albeit slight, *decrease* in mean arterial blood pressure", Epstein et al. 1978, pg. 495 [*italics in original*]).

These changes do not happen quickly, as Morgan suggests and as her scenario requires. They also don't happen unless you're in water up to your neck (or higher),

which is also a problem for the wading aquatic ape. For the changes to have a helpful effect when we stand up, these bipedal hominids would have to always enter the water by crawling (or maybe scooting in on their rear ends) and then stand up only after an hour or so. Hard to see where that helps you in the common AAT/H scenario of running into the water to escape predators. Maybe they could scoot *really fast*.

References

1993 *Review of Medical Physiology* by William F. Ganong, M.D. (UCSF), Appleton and Lange: Norwalk, Conn. 16th Edition. (chart on pg. 343)

1984. "Renal, endocrine, and haemodynamic effects of water immersion in man", by Murray Epstein, *Contributions to Nephrology* vol. 41, *Cardiocirculatory Function in Renal Disease*, pp. 174-188.

1978 "A Kinetic Assessment of Aldosterone Responsiveness in Secondary Hyperaldosteronism and in Anephric Man", by Murray Epstein, Edgar Haber, and Richard Re, in *The Endocrine Function of the Human Adrenal Cortex* vol. 18, pp. 493-508.

Another interesting fact is that the reflex which causes all the above (the Henry-Gauer Reflex) is not present in seals. It's highly likely that it isn't present in other aquatic mammals, but that hasn't been studied, as far as I know. However, Morgan thinks it's evidence of an aquatic past, when the reality is that this is something we would expect to see eliminated by an aquatic past

2001. "Osmoregulation in Marine Mammals", by Rudy Ortiz, in *The Journal of Experimental Biology* vol. 204, 1831-1844.

By the way, in this section of *The Scars of Evolution* Morgan also attempts to contrast the hormone situation regarding adrenalin in humans with that seen in quadrupeds by intimating that other animals live free of stress by either predators or each other:

1990 *The Scars of Evolution* by Elaine Morgan. Souvenir Press: London.

pg. 33:

"A quadruped in a secure environment may spend weeks at a time stress free and with no need for these glands to be activated until the rutting season comes round."

Where on the actual earth would we find these quadrupeds which live in such a "secure environment" that they can "spend weeks at a time stress free and with no need for these glands to be activated"? why has there never been a *Nature* show or *National Geographic* article about this predator-free Nirvana? what about the squabbling and fighting that goes on over food and water in most species? has Morgan never even, say, fed ducks at a city park?

Fat and the AAT/H

The feature that apparently started the AAT/H ball rolling is fat. Hardy claimed, and Morgan and others have echoed, that fat in humans is an aquatic adaptation, modified by convergent evolution to be similar to fat in seals and other aquatic mammals. They claim that it is bonded to the skin and not internally, as in our primate relatives. They're wrong.

On the subject of fat, Elaine Morgan has taken a route like that she's taken with salt and tears, finding a leading expert on the subject and then completely misrepresenting what that expert says. The expert Morgan uses here is Caroline Pond, who is accurately described as *the* leading authority on fat and its evolutionary significance in humans and other animals. (I've included excerpts from a few of Pond's works after this section; also check out her terrific 1998 book, *The Fats of Life*.) Morgan uses Pond's observations that humans are on the fat end of the scale compared to other mammals as her basis, but ignores Pond's observations that both quantity and amount of fat in humans is similar to that of captive monkeys if they aren't kept on a strict diet.

She also quotes Pond on the number of adipocytes (fat cells) but seems to not understand what Pond was getting at when she made this statement. This is the statement referred to when you see various AAT/H proponents talking about "10x as many fat cells" or, erroneously, "10 times fatter". The implication (sometimes said explicitly) is that we are 10 times fatter or necessarily have 10 times more fat cells than our primate relatives or some unspecified "savanna" animal. This is not what Pond was saying at all. Pond's article was aimed at pointing out that the usual models for human fat studies -- rats and mice -- are inappropriate because they have relatively large adipocytes which then expand and reduce as total fat goes up and down. Humans, as with other primates, have relatively small adipocytes and we tend to add more since they can't expand as much as those of rats and mice. This makes it harder to lose total fat compared to those rodents. Pond pointed out that this feature, relatively small and numerous adipocytes, is common to humans, fin

whales, hedgehogs, monkeys, and badgers; Morgan ignored all but the "whales" part of that statement.

Pond has also pointed out that human fat distribution indicates that it was not part of an aquatic adaptation. Such an adaptation is seen in whales and seals, but not in humans. Morgan has said that fat was an adaptation for insulation in an aquatic environment. Pond points out that fat is not adapted as insulation, although she also points out that this idea is fairly well entrenched, even in many physiology texts. For one thing, the distribution of fat, even in arctic species, doesn't indicate insulation; the fat layer is quite thin in some areas of the body of all species, even ones, such as seals, which are around 50% fat. She also notes that the subcutaneous (just beneath the skin) fat is the first fat to be used up, even in winter, and even in arctic animals, which is the opposite of what would be expected if insulation were a major purpose for fat. It plays, she suggests, no more than a minor role. The major role of fat, she feels, is as a food supply; this fits with how it's used up. The reasons for the differences seen in fat distribution in different species seem to be for shaping. Humans have the shapes they have due to fat distribution due to sexual selection; this of course makes sense in any species where fat distribution differs between the sexes; females are also much fatter than males. These sex differences make no sense if both sexes are "using" that fat (quantity and distribution) in the same way, as we would have to be for it to be an aquatic adaptation shaped by natural selection via the principle of convergent evolution. In fat aquatic species like the whales and seals Hardy and Morgan incorrectly say we resemble, both sexes are shaped basically the same, and their fat distribution seems to be primarily for streamlining while swimming. Humans and these aquatic species are radically different in their fat distribution.

Another obvious and huge difference ignored by Hardy, Morgan, and other AAT/H proponents is that the aquatic mammals they say we resemble also have fat life histories which are radically from humans; they rapidly gain fat while very young, and at a very early age they are essentially like their parents in their fat distribution and quantity. We start off fairly fat, drop within a few years to the leanest condition of our lives as children, and then rapidly build up fat at puberty, with radical differences in quantity and distribution of fat between boys and girls, and to top it off, at middle age our fat distribution changes once again. These are classic telltale signs of a trait which has been shaped by sexual selection rather than by adaptation to our environment. All this is very unlike those aquatic mammals whose fat has developed in such a way as to help them deal with that environment. For these human fat characteristics to be due to an aquatic adaptation, we would have to be aquatic as babies, non-aquatic as children, aquatic again in puberty, and even more aquatic in our old age. And females would have to be far more aquatic than males,

but only from puberty on. It just doesn't make sense as an aquatic adaptation, but it makes perfect sense as a feature developed as a result of sexual selection.

Also valid is the question of how likely it is that fat necessarily shows adaptation to an aquatic lifestyle. It does in seals and whales, animals which have been aquatic for tens of millions of years, where it shapes their bodies to streamline them for swimming speed. But does it necessarily show such an adaptation in aquatic animals versus their terrestrial relatives, as the AAT/H suggests? Studying bears answers that question, and Pond has studied a lot of bears. The polar bear is well enough adapted for aquatic life to be able to swim easily at enormous distances from land, diving and catching food underwater. They also show physical adaptation to aquatic life in their relatively small, streamlined head compared to their relatives (one of those ubiquitous aquatic adaptations absent in hominids I mention in the "Relevant Questions for the Aquatic Ape Theory" section). According to the AAT/H idea that fat differences between such animals are due to living in an aquatic environment either full or part time, one would expect polar bears' fat quantity and distribution to show differences from their terrestrial relatives. It doesn't. This is just one of many scientific tests of the AAT/H which it fails.

Another thing Pond emphasizes is that studies on living mammals suggest that fat (contrary to AAT/H claims) isn't an especially good insulator; these mammals rely on building a layer of stagnant air or water around their bodies rather than on their fat layer. Earlier studies by Scholander et al. which Morgan has used for her "evidence" (ignoring the later studies) were conducted with the skin of dead seals and polar bears, and of course do not accurately reflect the way in which living tissue attached to a living animal works. Later evidence using temperatures taken at the skin and the body core of seals show only about a 5 degree C difference -- helpful but minor compared to other factors. This fact has been pointed out to her.

Morgan has professed to being perplexed as to why human babies are so fat; she can't see any advantage to it outside of water. However, as anthropologist Jane Lancaster has pointed out, the fat built up in women for nursing, and that seen in babies, is most likely correlated with the massive brain growth seen in human infants. This brain growth is pretty much off the scale compared to other animals. So rather than the AAT/H proponents' surprise at this feature of human babies, one would be surprised *not* to find such a helpful feature to aid in this postnatal brain growth. It's also not surprising that it's unique among animals, since we also find our infant postnatal brain growth is unique among animals.

Finally, note that accurate measurements of fat in humans have mostly been on people who have plenty of access to food at all times, and this tends to make them fatter. Pond found that even northern Canadian natives who live on high meat diets generally are less fatty than Canadians who live in cities. It seems likely that

measuring the fat of modern humans in food rich situations does not give a accurate model for the rudimentary tool-using populations of 5 or more million years ago. Even early *Homo sapiens* and Neanderthals would likely have more steady, fat rich food supplies than those earliest ancestors. Using us to model them, as the AAT/H does, is very unlikely to be accurate when it comes to fatness. Using a combination of us and our similarities to other primates, which Pond documents, makes sense, but does not support the AAT/H.

So, why *can* we get fat? This feature is shared with our primate relatives. Why this is a shock to AAT/H proponents is, frankly, a mystery to me.

Why *does* our species get fat? It seems the answer to that is because we can. Having a relatively large amount of fat -- that is, the amount of fat which we see in normal, non-obese humans -- is useful in an evolutionary context, as long as it doesn't impair you too much. It gets you by in times of little food, and provides a cushion, so to speak, in times of illness. This is true for all animals. But too much fat is a hindrance, and one of the biggest problems influencing this is trying to avoid predators. It's been found that animals in places with fewer predators (some islands, for instance) tend to get fatter than their relatives in more predator-rich areas. (Predators have been a relatively minor problem for humans since we developed the controlled use of fire and effective weapons around a million years ago.)

Caroline Pond has shown this in studies on reindeer, which back up other studies showing the same thing. This excerpt from her university's site tells the story:

"Separated from the mainland perhaps 40,000 years ago by rising sea levels, and free from the need to gallop swiftly or be eaten, the Svalbard reindeer have evolved into a sub-species which is fatter, shorter and, frankly, dimmer, than the mainland reindeer. They need to get fat because it provides them with a fuel supply to carry them through the winter, when even the poisonous plants, which Svalbard reindeer have evolved to digest, are in short supply.

"Norwegian mainland reindeer would probably like to get fat too, but this would make them inefficient runners, a luxury they cannot afford.

"Having to run away from wolves keeps them slim," explains Dr Pond, who has made several trips to north Norway and Svalbard to study the reindeer in collaboration with scientists from Tromso University."

The Open University Alumni On-line Community, *Open Eye On-line*, "Reindeer" December 2000

1987 "Fat and figures", by Caroline Pond (Ph.D., Senior Lecturer in Biology at the Open University in Milton Keynes) *New Scientist* 4:62-66.

pg. 63:

"More than half the 31 captive monkeys that we examined were less than 5 per cent fat, thinner than most laboratory rodents, although all of them had continuous access to food and little opportunity to exercise. However, a few of the monkeys kept in the same way became obese, and at more than 25 per cent fat, were fatter than normal rats. Males and females were equally fat. In contrast, a typical "reference man" is estimated to be 15 per cent by weight adipose tissue, and a "reference woman" 27 per cent."

pg. 63:

"Thus most human beings are not only much fatter than most wild and captive mammals, but women and girls are consistently fatter than men and boys."

pg. 63:

"Both "normal" humans and exceptionally obese monkeys have large quantities of adipose tissue inside the abdomen, and the superficial depots around the limbs spread out to form an almost continuous layer."

pg. 63:

"The main differences between ourselves and other primates arise from the *quantity* of adipose tissue in humans. The *distribution* of human adipose tissue is similar to that of exceptionally obese monkeys. We don't need to postulate that special evolutionary forces are behind more than minor changes in the shapes of certain depots, such as the breasts (see Box, page 64)."

pg. 64:

"But, however you compare them, *Homo* is clearly the odd man out. In proportion to body mass, we have at least 10 times as many adipocytes as expected from this comparison with wild and captive mammals. Humans easily surpass such notorious fatties as badgers, bears, pigs and camels, and are rivaled only by hedgehogs and fin whales, in their deviation from the general trend, indicated by the regression lines on the figure."

pp. 64-65:

"But our recent observations on captive monkeys suggest that adipose tissue may grow in very different ways in rodents and in primates. The obese monkeys had more than 10 times as many adipocytes as slim monkeys of the same age, but the average sizes of the cells were similar in specimens of 5 per cent fat and those of

25 per cent fat. The fatter monkeys simply had *more* adipocytes; increases in the volume of cells contribute little to the growth of adipose tissue. The cellular mechanism of growth of adipose tissue in monkeys is thus fundamentally different from that of young laboratory rodents. Adipocytes probably do not normally expand more than about fourfold, but there is no obvious upper limit to the numbers of new cells that can be formed. Perhaps that is why the fatness of well-fed monkeys and humans is so much more variable than that of laboratory rodents, and why we primates are more susceptible to massive obesity than rats and guinea pigs."

pg. 65:

"On present knowledge, fin whales, hedgehogs, monkeys or possibly badgers would be better than rats as animal models of human obesity, because they develop relatively large numbers of adipocytes."

pg. 66:

"The study of other naturally fat mammals suggest that the anatomy of human adipose tissue, which contributes so much to the figures of both men and women, is not unique, nor necessarily a relic from an aquatic past (see Box)."

1987 "Not an aquatic ape -- just an exceptionally fat mammal", by Caroline Pond (Ph.D., Senior Lecturer in Biology at the Open University in Milton Keynes) *New Scientist* 4:64-65.

pg. 65:

"They think that the hair, skin and superficial adipose tissue of humans evolved into "blubber", similar in function to that of seals and whales. Let's consider the facts about the quantity and arrangement of adipose tissue in some aquatic mammals. In specialized aquatic mammals, such as whales, seals and manatees, the limbs are reduced or absent and the trunk is smooth and tapered. But whales and seals are not always exceptionally fat. Massively thick limbs or a bulging abdomen would spoil the streamlining. Adipose tissue around the guts and kidneys is greatly reduced. In all but the most emaciated terrestrial mammals, the mesentery that holds the gut in place is clouded with adipocytes. But in seals, even those that are 50 per cent fat, there is so little adipose tissue in the mesentery that you can read a newspaper through it. The superficial adipose tissue spreads over the trunk forming a continuous layer of blubber that may facilitate rapid swimming by acting as a shock absorber in turbulent water."

pg. 65:

"Less specialized aquatic mammals, such as otters, have elongated bodies and webbed feet, and but the limbs are relatively long limbs [sic] and they can travel

some distance over land. The distribution of adipose tissue in otters is almost identical to that of their terrestrial relatives. No one can claim that the limbs and trunk of humans have evolved further towards fully aquatic habits than those of the otter. Why should humans have adipose tissue like that of a highly specialized aquatic mammal? Anyhow, as most of us know only too well, there is plenty of fat inside the human abdomen. Badgers, hedgehogs and hamsters are among the few common wild mammals that normally accumulate large quantities of fat. In such species, the superficial depots enlarge disproportionately as they fatten, so that specimens above about 15 per cent fat -- the same fatness as "slim" humans -- seem to have an almost continuous layer of adipose tissue between the skin and the muscles. In other words, humans are just one example among several naturally obese mammals in which the superficial adipose depots are relatively massive. We can explain the contrasts between the arrangement of adipose tissue in humans and rats without postulating an aquatic ancestry for humans. The apparent shift in distribution is a direct consequence of the much greater abundance of adipose tissue in modern human beings. We would expect women and girls to have proportionately more of their adipose tissue in superficial depots than boys and men because, unlike most wild mammals, human females are usually fatter than males of the same age and lifestyle."

Pond, Caroline M.

1991 "Adipose Tissue in Human Evolution", pp. 193-220. *The Aquatic Ape: Fact or Fiction?* Edited by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds. Souvenir Press: London.

pg. 193:

"The sparse data on the 'natural' distribution and abundance of adipose tissue in primates show that the basic anatomy of human adipose tissue is similar to that of terrestrial monkeys, and so was probably inherited directly from their primate ancestors."

"Anatomical, ecological and biochemical information provides no evidence that the distribution of adipose tissue in modern humans has evolved as an adaptation to thermal insulation, as required by the Aquatic Ape Theory, or as protection from mechanical damage."

pg. 206:

"These data do not support the hypothesis that the large quantities of adipose tissue in superficial depots are adapted to function as thermal insulation. The biochemical properties of the subcutaneous adipose tissue also suggest that they contribute little to passive insulation in the arctic species (Pond et al., 1991b)."

1992 "Allometry of the distribution of adipose tissue in Carnivora", by Caroline M. Pond (Ph.D., Department of Biology, The Open University, Milton Keynes) and Malcolm A. Ramsay (Department of Biology, University of Saskatchewan, Saskatoon). *Canadian Journal of Zoology* 70:342-347.

pg. 342 (from abstract):

"There is no evidence for adaptation of the gross anatomy of the adipose tissue of polar bears to their semi-aquatic habits or arctic habitat."

Breath-holding, the descended larynx, the diving reflex, and the AAT/H

A common, ongoing claim by most AAT/H proponents is that only humans and aquatic animals can hold their breath, that the human descended larynx (compared to other mammals) is an aquatic trait, and that only humans and aquatic animals exhibit the diving reflex. This, they say, is evidence that we evolved in a situation with a lot of diving going on, ie. aquatic. However, even minimal study of the claim by AAT/H proponents would have revealed to them that the diving reflex is actually a universal trait found in *all* vertebrates, and that non-human, non-aquatic animals can and do hold their breath. I wouldn't hold your breath waiting for AAT/H proponents to stop making this claim, though, cause for years it's been pointed out to them and they haven't stopped yet.

Breath-holding in non-human animals

So contrary to the AAT/H claim, humans are not the only non-aquatic mammal which can hold its breath. Various monkeys, for instance, can and do hold their breath, and so do dogs. (Another common and related AAT/H claim is that non-aquatic animals have no control over their vocalizations, which should also surprise any dog owner.)

Lin (1982) reported on bradycardia studies with dogs, using dogs which showed an ability to hold their breath. ("Breath-hold Diving in Terrestrial Mammals" by Yu-Chong Lin (Department of Physiology, John A. Burns School of Medicine, University of Hawaii, Honolulu, Hawaii). In *Exercise and Sport Sciences Review*, 1982, vol. 10, pp. 270-307.)

By the way, seals and whales don't hold their breath when they dive; they store oxygen in their blood, and actually expel the air from their lungs as they dive. This system -- radically different from humans -- is used by pinnipeds and cetaceans.

The Descended Larynx and the AAT/H

Contrary to the claims of AAT/H proponents, a partially descended larynx -- in its initial evolutionary stages -- would be helpful when forming ever more complex sounds for communication, just as Darwin showed a rudimentary eye would be a helpful adaptation which could lead to a more complex "modern-style" eye. The descended larynx of humans is not fully descended at birth; instead it gradually descends during the first four years of the infant's life.

The descended larynx of humans therefore:

- A) undergoes this change during precisely the period when infants learn to speak -- in an ever more complex manner; and
- B) isn't fully formed for the first four years of the infant's life.

If it *were* an adaptation which "aquatic" hominids had developed from living in their environment, these "aquatic" infants would without its supposed benefits for the first four years of their lives. And of course, since one of the unfortunate consequences of this feature is that we can get water into our lungs more easily, it's difficult to see why a feature which promotes drowning would be considered "aquatic".

The idea that this is an aquatic feature also doesn't explain why, in human males, the larynx descends even further at puberty (that's why male voices get dramatically lower at that time). A far more sensible reason, based on research rather than speculation, will show up in a few paragraphs.

Even some AAT/H proponents, such as Marc Verhaegen, have realized that this feature is not at all like the larynx of the vast majority of aquatic mammals, although he still has described it as an "aquatic" trait. When Elaine Morgan acknowledged Verhaegen's finding this out, she also admitted she just hadn't looked at the evidence on this -- excuse me? didn't look at the evidence? (that isn't someone I'd want to buy a used theory from...)

Morgan has stated that the descended larynx indicates habitual mouth breathing, which she says, without evidence, would be better for diving, but we see that other terrestrial mammals mouth breathe just as humans do when exerting themselves -- common mammals like dogs and cats, so that doesn't add up. Other than that, it's

merely suggested that it *must* be an aquatic trait because it's seen only in humans and (a very few) aquatic mammals. In short, there is no good explanation given why this would be an aquatic trait, and it turns out that there's excellent reasons to see that it isn't, for instance, the fact that, contrary to common AAT/H claims, there are other terrestrial mammals with descended larynxes.

Previously on this page I had mentioned a problem concerning the descended larynx as a feature of our transitional population (as Morgan claims it) -- that the skulls of hominids after the transition don't show the basicranial angle we see in humans that indicates a fully descended larynx. It occurred several million years later. However, it's possible that a descended larynx wouldn't leave this mark in the basicranial angle, but even this does nothing to comfort the AAT/H proponent who uses the larynx as an example of an aquatic trait, because it turns out that there are several terrestrial mammals (and even some birds) with permanently descended larynxes, and even more with larynxes that habitually descend when vocalizing.

For instance, chimpanzees have been found to have partially descended larynxes -- that's right, our closest relatives are similar to us in this feature, although their hyoid (the U-shaped bone that supports the tongue muscles) doesn't also descend as ours does. (This gives us more fine control of vocalization than they have.)

Nishimura, Mikami, Suzuki, and Matsuzawa explain in the abstract of their 2003 paper ("Descent of the larynx in chimpanzee infants"):

We used magnetic resonance imaging to study the development of three living chimpanzees and found that their larynges also descend during infancy, as in human infants. This descent was completed primarily through the rapid descent of the laryngeal skeleton relative to the hyoid, but it was not accompanied by the descent of the hyoid itself.

But red deer and their North American relatives the wapiti, as well as fallow deer, have larynxes that are at least as descended as humans on a permanent basis, and more so when vocalizing. It turns out that a lot of mammals' larynxes descend a lot when vocalizing, which shows us how such a feature could arise. Others whose larynxes descend while vocalizing include koalas, "lions, tigers, and other members of the genus *Panthera*" (Hauser and Fitch 2003), dogs, pigs, goats and monkeys (Fitch 2002), and even at least some birds, like roosters and cardinals (Riede and Suthers, see refs). It seems likely that as more animals are tested, we'll find still others with this feature. W. Tecumseh Fitch, perhaps the leading researcher on this subject, explains why this wasn't seen decades back when Sir Victor Negus did his research:

Comparative Vocal Production and the Evolution of Speech: Reinterpreting the Descent of the Larynx (2002)

W. Tecumseh Fitch:

9th pg.: The work on animal vocal anatomy dates mostly from the 19th century, and was based exclusively upon dissections of dead animals. By the time new techniques allowing anatomical visualization of living animals were developed, such as x-ray film (cineradiography) or MRI, the study of comparative anatomy had fallen from favor. Although occasional critics pointed out that the crucial issue is not the resting position of the larynx, but its position during vocalization (Nottebohm 1976), it is only recently that modern imaging techniques have been applied to live vocalizing mammals (Fitch 2000a). These data indicate that mammals lower the larynx as a matter of course during vocalization, in some cases approaching the "two-tube" configuration typical of adult humans. Furthermore, new anatomical analyses show that humans are not unique in our laryngeal position, because several other species also have a permanently descended larynx.

Comparative Vocal Production and the Evolution of Speech:
Reinterpreting the Descent of the Larynx W. Tecumseh Fitch

more from the same source:

9th pg.: These data indicate that the vocal tract configuration of vocalizing animals, at least in dogs, pigs, goats and monkeys, is more similar to that of human talkers than was previously inferred on the basis of dissections of dead animals. In particular, all four nonhuman species examined can and do lower their larynges into the oral cavity during loud vocalizations, either to a relatively minor degree (goats) or to a surprisingly extensive degree (dogs).

W. Tecumseh Fitch

still more from the same source:

11th pg.: Finally, these data suggest that a lowered larynx during vocalization is in fact a primitive trait that we share with other mammals, rather than a uniquely human adaptation. What is unusual about our species is that the adult human larynx is permanently lowered, rather than dropping only during vocalization. However, other recent data show that even this difference is not uniquely human.

W. Tecumseh Fitch

and a bit more from the same source:

11th pg.: But despite the claims of Negus (1949), this position is not unique to humans. In at least two species of deer, red deer *Cervus elaphus* and fallow deer *Dama dama*, the larynx of postpubertal males is permanently lowered to a resting position comparable to that in humans.

W. Tecumseh Fitch

Why does this feature arise? Fitch has done a lot of work on this subject and has a lot of what seems to me to be excellent data strongly suggesting it's because it allows the animal to exaggerate its size when vocalizing. This can be helpful in a variety of situations; for instance as a mating display (as especially seen, or rather heard, in the deer species he's studied) or other "status" displays, or quite possibly in scaring off predators. The lowered larynx makes for a larger volume in the vocal apparatus and thus the tone is lowered -- just as a larger drum sounds deeper or a woodwind or brass instrument gives lower sounds when the internal volume is changed by moving keys or valves. The research of Fitch and others shows that animals do make judgments about body size based on these audio cues.

How about the diving reflex: are humans well adapted for diving?

Yes, we know humans *can* swim and dive, but so can other non-aquatic animals. Do humans show the adaptations that are found in animals with a lifestyle in which diving is important, as the AAT/H says it was for us?

AAT/H proponents say they do, and that other terrestrial mammals don't show such responses as breath-holding and the "diving" reflex. The following scientists' writings show that the facts say otherwise. Note also that although Hardy said in 1960 that the diving reflex was only found in humans and diving animals, contrary evidence had been available for over 20 years before that.

1982 "Breath-hold Diving in Terrestrial Mammals" by Yu-Chong Lin (Department of Physiology, John A. Burns School of Medicine, University of Hawaii, Honolulu, Hawaii). In *Exercise and Sport Sciences Review*, vol. 10, pp. 270-307.

pg. 271: However, universality of the "diving" responses is well recognized in that these responses exist in all vertebrates studied so far, differing only in intensity and tempo.

pg. 272: Many previous reports indicated that there is a slow development of bradycardia response in humans. Undoubtedly, these reports refer not to the onset but to the slow attainment of the maximum bradycardia response. It should be noted that diving mammals arrive at maximum bradycardia much faster.

pg. 277: In contrast to diving species, hypertension is a consistent finding in man during BH.

1983 *Diving and Asphyxia: A comparative study of animals and man* by Robert Elsner (Professor of Marine Science, University of Alaska) and Brett Gooden (Adelaide, South Australia. Formerly Lecturer in Physiology, University of Nottingham). Cambridge University Press: Cambridge.

pg. 20: Bradycardia during diving, breath-holding and asphyxia has been observed in many vertebrate species, including man. Among those studied are rabbit (Bauer, 1938), sloth (Irving, Scholander & Grinnell, 1942b), snake (Johansen, 1959; Murdaugh & Jackson, 1962), dog (Elsner *et al.*, 1966a), pig (Irving, Peyton & Monson, 1956), armadillo (Scholander, Irving, Peyton & Grinnell, 1943), echidna (Augee *et al.*, 1971) and lizards (Wood & Johansen, 1974). Terrestrial mammals generally do not respond as immediately nor as markedly as the aquatic species.

1990 *Diving Medicine*, Second Edition. by Alfred A. Bove, M.D. (Chief, Section of Cardiology, Temple University Hospital, Philadelphia, Pennsylvania) and Jefferson C. Davis, M.D. (Hyperbaric Medicine, P.A., San Antonio, Texas). W.B. Saunders Company: Philadelphia, London, Toronto, Montreal, Sydney, Tokyo.

pg. 66: Unlike in diving animals, arterial blood pressure seems to increase while cardiac output decreases only slightly during breath-hold diving in humans.

pg. 66: One of the most intriguing findings with regard to diving medicine is the occurrence of cardiac arrhythmias during breath-hold

diving. This is in contrast to diving animals who display a marked bradycardia but never show any cardiac arrhythmias during diving. In humans, it is common to find various types of arrhythmias even during simply breath-holding.

1992 *Diving and Subaquatic Medicine*, Third Edition. by Carl Edmonds, Christopher Lowry, and John Pennefather.

pg. 357: In humans, the diving reflex is more rudimentary and undeveloped. Although there is a diving bradycardia, it is often complicated by the development of idioventricular foci producing ectopic beats. ECG abnormalities are frequent during or after the dive. T-wave inversion, premature ventricular excitation and atrial fibrillation, together with other irregularities and dysrhythmias, are common.

pg. 358: The drop in cardiac output noted in the diving mammals is seen to only a slight degree in humans.

1990 *Sea Otters* by Marianne Riedmann. Monterey Bay Aquarium: Monterey, California

pg. 33: While diving for food, sea otters must conserve oxygen and contend with underwater pressure. They generally forage in shallow waters of less than 60 feet, and their feeding dives usually last one to two minutes. Yet sea otters have been known to dive to depths of 330 feet and remain under water for up to four or five minutes. In contrast, a person without oxygen passes out within three minutes. Sea otters can hold their breath for such a long time in part because their lungs are nearly two-and-a-half times larger than the lungs of a similar-sized mammal. Large lungs help store oxygen and regulate buoyancy while the otter floats on the surface. And sea otters have flexible ribs that allow the lungs to collapse under pressure. Otters also have cartilaginous airways connected directly to tiny, air-filled lung sacs which help provide an unrestricted flow of oxygen to the blood. In addition, the sea otter's blood has a higher buffering capacity than that of non-diving mammals, which helps the otter handle the excess carbon dioxide that accumulates under pressure during a dive.

1985 *The Natural History of Sea Otters* by Paul Chanin. Croom Helm: London and Sydney.

pp. 21-22: Like seals, sea otters have a higher concentration of haemoglobin than terrestrial mammals (and river otters) which enables them to carry more oxygen in their blood. They also have relatively large lungs which, compared to other marine mammals, are twice as large as expected and are the reason for the sea otter's large rib cage. The lungs may form an important oxygen store but it has been suggested that their importance is in providing buoyancy rather than an air supply. This is very necessary in an animal which has little or no fat to give buoyancy and which often carries large stones with it when feeding and foraging (see Chapter 3).

1990 *The Pinnipeds: Seals, Sea Lions, and Walruses* by Marianne Riedmann. University of California Press: Berkeley, L.A., and Oxford.

pp. 28-29: Bradycardia and the channeling of blood to critical organs save oxygen. Since muscles are deprived of their usual supply of blood and oxygen, they compensate by containing large amounts of a special compound called myoglobin, an iron-bonding pigment related to hemoglobin that stores oxygen. Myoglobin helps the seal to tolerate the large accumulation of carbon dioxide in its bloodstream that prompts breathing during periods of prolonged breath-holding. Myoglobin therefore helps the animal to conserve oxygen during high-speed movement or deep dives. In fact, cetacean and pinniped muscles appear nearly black when they are exposed to the air. This dark coloration is due to the extremely dense concentrations of deep red myoglobin in the muscle tissues.

A seal's muscles are also able to handle the high amounts of lactic acid that accumulates in its system during periods of heavy or prolonged activity (such as diving), which causes muscle exhaustion in land mammals. The muscles of marine mammals can function with insufficient oxygen for many hours. Most of the time, the muscles function aerobically (with oxygen), even though the seal may be diving and exercising heavily. They function anaerobically when oxygen supplies are depleted by prolonged submergence. When a seal surfaces from a long dive, it takes several rapid and deep breaths until the body's oxygen supply is replenished; at the same time, at least in harbor seals, a

pronounced tachycardia (extremely rapid heartbeat) occurs (Fedak et al. 1985). Seals and whales even have an automatic mechanism that cuts off breathing if they are knocked unconscious, so that their lungs will not fill with water.

In pinnipeds, the oxygen-transporting circulatory system is very large. A seal total blood volume in relation to its body weight is 1.5-2 times greater than that of other mammals. According to Scheffer (1976), "In a full-grown walrus the forked veins which drain the lower body are so enormous that a man can draw them over his legs like a pair of pants!" Wickham et al. (1985) and others (e.g., Lenfant et al. 1970) have identified a number of characteristics of phocid seal blood that appear to help the seal to cope with the hypoxia, or oxygen deficiency, of long dives. Phocid blood has a greater capacity than the blood of terrestrial mammals to store oxygen because phocids have fewer and larger red blood cells with a higher concentration of hemoglobin, which stores and carries oxygen. And despite the high viscosity, or thickness, of phocid blood, the rate of blood flow is high.

[My note: Since "Myoglobin helps the seal to tolerate the large accumulation of carbon dioxide in its bloodstream that prompts breathing during periods of prolonged breath-holding" why wouldn't people have this handy feature, which would keep them from breathing (and drowning) when they know perfectly well they shouldn't, ie. when under water for too long.]

1990 *The Natural History of Seals* by W. Nigel Bonner. Facts on File: New York, Oxford, and Sydney.

pp. 33-34: Breath-hold capacity can be increased by taking more oxygen down with each dive. A human diver will breathe deeply -- hyperventilate -- before diving, and descend with lungs full of air. There are disadvantages in this. For a start, full lungs increase buoyancy and so make it more difficult to descend; secondly, there are pressure problems associated with diving with full lungs. Seals hyperventilate before diving, but they expel most of the air from their lungs before they go down, and hence they do not depend on air from the lungs for their oxygen when they dive.

Besides being stored as a gas in the lungs, oxygen can be carried in physical solution in the blood and tissue fluids, or chemically bound to

haemoglobin in the red blood cells or to another respiratory pigment, myoglobin, in the muscles. In seals the major store is carried in the blood. Seals have greater blood volumes than terrestrial mammals. A Weddell seal, for example, has about 150 ml of blood per kilogram bodyweight, about twice the value for Man. Furthermore, seal blood contains more haemoglobin than human blood -- about 1.6 times as much. The combined result of this is that the blood oxygen stores per unit of body weight is about three or three and a half times that of Man. Gerry Kooyman (1981) has pointed out that because blubber is more or less inert and represents about 30 percent of the seal's body weight, a more realistic comparison might be to lean body weight, in which case a seal's blood oxygen store would be about 5.3 times that of Man (though it should be borne in mind that an obese human may have about the same proportion of fat -- though differently distributed -- as a seal). Not only does the seal have more haemoglobin in its blood than Man, it also has very much more myoglobin in its muscles. This protein, which resembles haemoglobin in its ability to combine loosely with oxygen, is what gives muscles their red color. [my notes here unfortunately left out a partial sentence. sorry; the following statement refers to the muscles of seals] Weddell, containing about ten times the myoglobin concentration of human muscle, is almost black.

Update note: some proponents of the AAT/H have moved away from the idea that the aquatic phase was in a salt water environment, but most still claim humans' sweat and tears systems evolved as an aquatic feature, as an excretory system for salt. This is a remarkable development, and demonstrates one of the reasons the AAT isn't taken seriously by those who study the evidence. If tears and sweat in humans are claimed to be due to an aquatic lifestyle and the need to excrete salt, the aquatic lifestyle must have been in a salt water environment. A freshwater aquatic period would be no more or less likely to produce these adaptations than a terrestrial lifestyle, so the purported aquatic lifestyle doesn't explain the supposed adaptation of sweat and tears as systems to excrete salt. Of course the AAT/H proponents are wrong about what the human sweat and tears systems can do, so that just compounds their error.

Tears and the AAT/H

"Until evidence of other animals shedding emotional tears is well documented, I will continue to maintain that emotional tearing generally occurs only in humans. I agree with Montagu's view that 'the shedding of tears as an accompaniment to emotional distress has been attributed to other animals... The truth, however, appears to be that while some of these animals may on occasion exhibit the

evidences of tears, this occurs very seldom, and is the exception rather than the rule. ...Psychic weeping is not known to occur as a normal function in any animal other than man." (William H. Frey 1985: 147. *Crying: The Mystery of Tears* by William H. Frey, Ph.D. with Muriel Langseth. Minneapolis: Winston Press.)

Some of you may recognize the name of the quoted above; he's the guy Elaine Morgan correctly refers to as "a leading present-day authority on the subject" (Morgan 1990: 98, *The Scars of Evolution*). Dr. Frey has done studies of emotional tears and Morgan feels they support her theory. Why she feels they support her is rather inexplicable, once you look at the evidence, including the evidence in the book which Morgan cites.

This particular AAT/H idea, initially promoted by Morgan and since repeated many times by AAT/H proponents, is that emotional tears are seen only in humans and aquatic animals (she cites Frey as proof despite his conclusion, which I quoted just now) and they (and our sweat glands) evolved as system to excrete salt. To do this, she ignores a basic principle of physiology I've described below in a section on [homeostasis](#).

This section is in two parts: this first part is specifically on evidence for emotional tears in non-human animals. The second deals with tears and what they contain, and what this means in regard to the likelihood of tears being an excretion system.

Do Animals Shed Emotional Tears?

This is the title of chapter 14 in William Frey's book (Morgan cited it in *The Scars of Evolution* but left off the main title, leaving only the sub-title, and also left off Frey's co-writer). In it Frey looks at evidence, both scientific and anecdotal, for emotional tearing in non-human animals. His conclusion opened this page.

From Frey's Chapter 14: Do Animals Shed Emotional Tears?

from pp. 135-139: **"Some Scientists Say Animals Cry Tears"**

In this section Frey mentions "several reports of animals shedding emotional tears -- or what were interpreted as emotional tears -- in the scientific literature cannot be ignored."

Frey opens with Darwin's reports of tears in Indian elephants. Frey also points out the information that AAT/H proponents never seem to mention (even though it appears on the same page of Darwin's *Expression of the Emotions in Man and Animals*); that Darwin reported contrary information from his correspondents in

Ceylon, where observations of recently captured elephants showed no tearing, and "the native hunters asserted they had never observed elephants weeping".

Then Frey continues with reports of other animals shedding tears: dogs and wolves (Cecil Reynolds 1925); seal (Ronald M. Lockley 1966 *Grey Seal, Common Seal*); sea otter (Georg W. Stellar); lab rats (red tears from Harderian glands [the same types of glands that elephants have]) (John E. Harkness and Marcella D. Ridgeway); and gorilla (Dian Fossey 1983 *Gorillas in the Mist*).

Note that already we see a mix of aquatic and non-aquatic animals mentioned; this will be a repeating theme, as you'll see. Another recurring theme, and the reason Frey ultimately does not accept these reports as documented, is that none of these reports, indeed none of the "pro-tears" reports, have any means of determining that these tears are due to emotion rather than stress or irritation. In any event, this already destroys the AAT/H claim that only aquatic animals have been reported shedding emotional tears. It should also be noted that Frey's book was cited by Morgan, who nevertheless somehow managed to skip all the reports of non-aquatic animals, despite the fact that they are interspersed with the reports of tears in aquatic animals, just as they are in my list in the paragraph above.

from pp. 139-141: **"What Animal Experts Say"**

In this section, Frey and his co-writer sent out questionnaires to zoos, animal parks, and marine parks, veterinarians, etc., to see if any of them reported ever seeing emotional tears in nonhuman animals.

Brian Davies, "an animal protectionist and executive director of the International Fund for Animal Welfare" (he's written two books protesting the harp seal hunts in Canada) says yes for seals. Wayne Franzen, trainer of Okha (an Indian elephant), and co-owner of Franzen Brothers Circus, says yes for Indian elephant.

Ian Douglas-Hamilton, "who has done extensive studies of African elephants replied, 'I cannot say that I observed emotional tears in African elephants, although I have seen tears appear when they have been shot or wounded. It is possible that these may be related to emotion, but I am sorry to say I really do not have enough observation in this to say one way or the other'."

Frederick A. King, director of Yerkes Primate Center in Atlanta and survey of 5 experienced primate scientists there say no.

The following experts were asked "(1) Have you ever seen nonhuman animals shed emotional tears? and (2) Do you know of any documented evidence of nonhuman animals shedding emotional tears?" Saul Kitchener, director of the San Francisco Zoological Park; R.M. McNorman, assistant curator of mammals, Bronx Zoo; G.

Styfer, overseer at the Toronto Zoological Park; Donald L. Jansen, DVM, associate veterinarian at the San Diego Wild Animal Park; Sanford Friedman, curator and chairman of mammal department at the Brookfield Zoological Park in Illinois "all responded no to both questions".

"Veterinarians and animal trainers who have worked extensively with seals, dolphins, and whales at Sea World in Orlando, Florida, the Seaquarium in Miami, Florida, Marineland in Los Angeles, California, and the Como Park Zoo in St. Paul, Minnesota" all said no. As they report, "seals, dolphins, and whales secrete a watery mucus to protect their eyes from sea water. The overflow of tears due to lack of a drainage system may have been misconstrued as emotional tears".

from pp. 141-146: **"Other Reports of Animals' Tears"**

Finally, since his earlier work was written about in *Psychology Today*, Frey has gotten many anecdotal reports about pets or other animals shedding tears. These animals include dogs, cats, cows, pigs, lambs, horse, and a kangaroo.

Summary:

So we can see that there is every bit as much, and as little, evidence for non-aquatic animals shedding emotional tears as there is for aquatic animals. This is contrary to Morgan's claims in print, as well as the claims Morgan and others have made repeatedly in the sci.anthropology.paleo newsgroup and other online forums.

We also see still more evidence of Morgan's style, deliberately burying contrary evidence that is in a book which she cites. (Unless we are to believe that when she read Frey's book, she didn't read the chapter entitled "Do Animals Shed Emotional Tears?" -- does *anyone* believe that's likely?)

This second part deals with tears and what they contain, and what this means in regard to the likelihood of tears being an excretion system.

When Morgan cites William Frey's work as support for the AAT/H; her reasoning is twofold:

1. She claims that emotional tears are shed by humans, aquatic animals, and Indian elephants, and by no other animals. Part one of my post showed that the evidence for this does not exist. There are no documented cases of emotional tears being shed by any nonhuman animal, and though there are anecdotal reports of

such tears in nonhuman animals, these report such tears in many non-aquatic animals. So it cannot be said to be a trait shared only by humans, aquatic animals, and Indian elephants.

2. Frey's work demonstrates that emotional tears do serve an excretory function, and Morgan claims that the original function of emotional tears was the elimination of salt.

First we have an interesting point; then we have to clear up some confusion introduced by Morgan's inaccurate terminology.

The point is that the composition of human tears *does show an excretory function of tears which sheds light on their environment of origin*. This feature of tears, that they are excretory in regard to some elements, was known even before Frey's work. The actual composition of tears, as opposed to some imagined or postulated composition, is not favorable to the AAT/H, but we'll come to that at the end.

The problem is the confusion Morgan has introduced regarding the types of tears. She uses the term "psychic tears", and posits these as being one of two types of tears, the other being "reflex". She claims there is "general agreement on this", but in fact she is confused. The two types of tears are "basic" (now generally called "basal" or "continuous" tears) and "reflex" tears, and there is in fact general agreement on this. Emotional, also called "psychogenic", tears are one form of reflex tears. The stimulus is emotional rather than being irritant-induced. Basal tears are the normal tear contribution to the "tear film" which lubricates the eye.

"Reflex secretion may be of *peripheral sensory origin* through fifth nerve stimulation (cornea, conjunctiva, skin, nose) or of *central sensory origin*. In the latter the stimulation may be retinal, varying with the intensity of light, or psychogenic, as in weeping caused by emotional disturbances or by various central nervous system diseases. The production of tears on an emotional basis is unique to humans among all vertebrates". (Benjamin Milder 1987:24)

The excretory abilities of the lacrimal (tear) glands

It is an *established physiological fact* that to be involved in changing the body's balance of a substance, such as salt (the balance in what we are talking about is the balance of water to a given substance), any given excretion must contain a concentration of the substance that is greater than its concentration in plasma, which is called being "hypertonic". An organ which does this is said to be capable of "active" rather than "passive" excretion for that substance. That the lacrimal glands

can and do perform active excretion is well known to those in the field of ophthalmology. However, they are not capable of actively excreting sodium, as the AAT/H requires.

Central to Morgan's thesis is that our lacrimal glands actively excreted salt at some time in the past, even though they don't now. This requires that our lacrimal glands underwent two massive changes, starting and ending with a passive secretion of sodium. This idea is certainly far less parsimonious than the idea that the observed and measured active secretion of the lacrimal gland indicates our actual environmental past habitat.

Secretions in tears

The facts on substances which the lacrimal gland is capable of actively excreting, and on sodium, which the lacrimal gland cannot concentrate sufficiently for active excretion.

"Sodium in tears is about equal to that in plasma and has been found to correlate with it, suggesting a passive secretion into the tears.

"Potassium, with an average value of about 20 mmol/l is much higher than the corresponding plasma concentration of about 5 mmol/l. This indicates an active secretion into the tears". (van Haeringen 1981:92)

"The manganese concentration of emotional tears (25.2 +/- 6.6 ng/ml) was not significantly different from that of irritant-induced tears (34.2 +/- 8.8 ng/ml) obtained from the same healthy female subjects". (Frey et al. 1981:564)

"The manganese concentration of emotional tears from women exceeded that of serum (0.98 +/- 0.09 ng/ml) obtained from the same subjects by about 30-fold". (Frey et al. 1981:564)

"Paired samples from 25 women also showed that the protein concentration of emotional tears was 24% greater than that of irritant-induced tears ($P < .01$) (Table 1). We found a higher protein concentration for emotional tears from men as well ($P > .05$), although we obtained paired samples from only four subjects". (Frey et al. 1981:562)

The only higher concentrations that Frey and his colleagues found being excreted in emotional tears as contrasted to irritant-induced tears were higher concentrations of proteins. Frey feels that the process of crying helps in emotional release through eliminating substances such as prolactin, ACTH, Leucine-Enkephalin, and manganese. He also feels that the levels of prolactin in one's system provide a

threshold effect, affecting how easily emotional tears are shed in response to a given stimulus. This, he thinks, accounts for his findings that women "shed tears more often and more readily than men". Adult women typically have much higher levels of prolactin than adult males, and these levels also rise during pregnancy, another period when emotional tears are known to be more easily shed.

Innervation of tear secretion

Morgan has suggested that the source of control for psychogenic tearing is important to her theory, but this is based on her mistaken idea that human emotional tearing is a characteristic shared by aquatic animals and no other animals. This has been shown to be a completely unsupported notion: there is no documented evidence for nonhuman aquatic animals shedding emotional tears, and the anecdotal and non-documented evidence is at least as strong or stronger for emotional tears in non-aquatic nonhuman animals.

What matters is the ability of lacrimal glands to excrete hypertonic substances. If they cannot excrete a given substance with a greater concentration than it appears in plasma (i.e., "hypertonic"), the purpose for these tears *cannot be for excretion of an excess of that substance*. It is well documented that human tears are *never* hypertonic in regard to sodium.

Tears are, however, *strongly hypertonic in regard to potassium*. In this regard, the lacrimal glands call to mind the "salt" glands of terrestrial reptiles and birds. Morgan has many times -- both in published work and online -- inaccurately claimed that salt glands are only found in marine birds and reptiles, ignoring the many terrestrial birds and reptiles who have these glands. These animals include ostriches and other birds, and lizards, including iguanas, chuckwallas, and others. The "salt" glands of these terrestrial birds and reptiles, unlike those found in marine birds and reptiles, are used primarily to excrete potassium. "In other words the secretion resembles that of the ostrich and differs markedly in composition from that of marine birds and reptiles" (Peaker and Linzell 1975:247-8).

So the actual observed and measured excretions in human tears far more closely resemble the excretions from the "salt" glands of terrestrial birds and reptiles than those of marine birds and reptiles. Thus for human tears' excretory abilities to indicate a terrestrial background requires no change. For their abilities to indicate a marine background requires not just one but two massive changes, starting and ending with a passive secretion of sodium.

The idea that human tears indicate a marine past is certainly far less parsimonious than the idea that the actual observed and measured active secretion of the

lacrimal gland indicates our actual environmental past habitat would be terrestrial rather than aquatic.

References

1975 *Salt Glands in Birds and Reptiles* by M. Peaker and J.L. Linzell (Dept. of Physiology, Agricultural Research Council, Institute of Animal Physiology, Babraham, Cambridge). Monographs of the Physiological Society No. 32. Cambridge University Press: Cambridge, London, New York, Melbourne.

1981 "Clinical Biochemistry of Tears" by N.J. van Haeringen, Ph.D. *Survey of Ophthalmology* 26(2):84-96.

1981 "Effect of stimulus on the chemical composition of human tears" by William H. Frey II, Ph.D., Denise DeSota-Johnson, B.A., and Carrie Hoffman, B.S.; and John T. McCall, Ph.D. *American Journal of Ophthalmology* 92:559-567.

1985 *Crying: The Mystery of Tears* by William H. Frey II, Ph.D. with Muriel Langseth. Minneapolis: Winston Press.

1987 "Chapter 2: The lacrimal apparatus" by Benjamin Milder, M.D. In *Adler's Physiology of the Eye: Clinical Application*, edited by Robert A. Moses, M.D. and William M. Hart, Jr., M.D., Ph.D. Eighth edition. The C.V. Mosby Company: St. Louis, Washington, D.C., and Toronto.

Homeostasis

Homeostasis in bodily fluids refers to a self-regulating process wherein an organism maintains, within reasonable limits, a relatively steady balance of a given substance. One of the most important of these is salt. Our plasma contains salt, and we need a certain amount of it or we get sick or die. This is not just true of blood plasma, but the fluid (cytoplasm) in all our cells. This isn't due to aquatic or non-aquatic lifestyles, it's common to all vertebrates. (Its only connection to aquaticness is that it's almost certainly a holdover from the extremely ancient past of hundreds of million of years ago when there actually were nothing but aquatic animals.) If we get too much salt in those fluids, we have to be able to get rid of some or we get sick or die. This also is true of all vertebrates, terrestrial and aquatic. Homeostasis in bodily fluids doesn't just pertain to salt and water; other elements, like potassium, are involved too. The AAT/H often focuses on this process

because how these systems work in different animals does say a lot about the environmental backgrounds over their evolutionary history. They are right to look at it, but sadly, they generally seem to lack a basic understanding of the physiology behind it, which leads them to make huge errors. Our method of maintaining balance in both salt and potassium, when compared to aquatic and terrestrial mammals, does indeed point to one of the two environments as the one we evolved in. But it isn't aquatic.

It is *an established physiological fact* that to be involved in changing the body's balance of a substance, such as salt or potassium (the balance in what we are talking about is the balance of water to a given substance), any given excretion must contain a concentration of the substance that is greater than its concentration in plasma, which is called being "hypertonic". An organ which does this is said to be capable of "active" rather than "passive" excretion for that substance. For example, while AAT/H proponents claim we used our sweat glands and tears to excrete salt, they are not capable, or even close to being capable, of active secretion. This is true of all mammals, including aquatic mammals. Only one organ in mammals is capable of actively excreting salt, the kidneys. This is why the kidneys of marine mammals are all large and heavily lobed, unlike human kidneys; with better hormonal control over rate of urine formation and concentration *via the kidneys*. Besides the aquatic marine environment, there is one other environment which needs this type of adaptation, and that, ironically, is very dry deserts. The problem in both places is the same, little or no fresh water, and the denizens thereof must be able to conserve the water they get and expel the salt they get with it. So kangaroo rats and whales turn out to have a lot in common, homeostasis-wise. Note that when I pointed this out in the sci.anthropology.paleo newsgroup, some AAT/H proponents (including Elaine Morgan) couldn't see why we would use the same system that *every other mammal uses*, without exception.

Because birds' and reptiles' kidneys are not as highly developed as those of mammals, they have had to develop the salt glands that AAT/H proponents think we should've emulated. Remember that an organism can't just evolve whatever feature it likes, like picking up a performance muffler and installing it on your car. They start with what they have, and birds and reptiles have kidneys which are inefficient compared to those of mammals. In fact, the more sophisticated kidney is considered to be one of the great mammalian adaptations. *All* mammals faced with high salt loads relative to fresh water availability, have developed specialized kidneys to deal with the problem, kidneys which can produce highly concentrated urine. Humans do not have these features. Aquatic mammals, in fact, can even get necessary water by drinking seawater for an indefinite period; humans doing so get sick and die. (Aquatic mammals actually generally get enough water through their food, which is also about as salty as seawater.)

Besides the simple yet critical matter of sweating being entirely incapable of producing the required hypertonic salt-water mix -- it's never even close to hypertonic -- there is another critical reason that sweating (whether eccrine or apocrine) is not a usable salt excretion system: its essentially unregulated nature (the mammalian renal system, on the other hand, is a highly regulated system whose purpose is the regulated excretion of various substances and the maintenance of the required salt-water balance). If you simply sweat a lot and drink fresh water, under work stress the renal system can shut down. This causes a health crisis, with accompanying violent and painful cramps, as well as -- less immediately dangerous but still critical -- fatigue and depression. This would make relying on "sweating plus drinking water" a highly dangerous form of salt excretion, even if the required large amounts of drinking water are available.

How large would these amounts of water be? Assume for a moment that you don't mind violent cramps on a daily basis, and that they wouldn't be a problem for an aquatic mammal. How much would you have to sweat to sweat out this salt? Some early researchers had the idea that sweat might be a excretory process rather than the one which tends to retain as much non-water as possible. They of course realized that salt was far too dilute in sweat for it to be a target of this hypothesized excretory system; they went for urea as the target. However, researchers J.S. Weiner and K. Hellman pointed out that the sweat glands' activity "is directed quite to the opposite purpose, of conserving sodium chloride and glucose" (1960:171) and they did the calculations to see how much sweat would be required to perform this task: the number is 80 liters. 80 liters of sweat every day, and the upper limit for men working all day in desert conditions (far more strenuous and harsh conditions than our hominid ancestors would go through) is 10 liters (6 liters being the more normal amount under those harsh conditions). Clearly, sweat just can't be expected to do the job, and keep in mind that urea is far more plentiful in sweat than salt is. And even if you could sweat more than 80 liters a day, every day, there's those violent cramps. Sweat as an excretory system just doesn't make sense at all. By the way, all this info was available long before even Hardy wrote up his AAT/H paper; even longer before Morgan did her first book on the subject. Was Morgan aware of it? I got the reference to Weiner and Hellman's research from her; she quoted them. So she had the info in her hand that proved her contention to be wrong even as she wrote it up.

Organisms which rely on maintaining a salt-water balance (all vertebrates except for some fish) must have a regulated system which doesn't simply "mess up" their salt-water balance. This isn't just a nicety, or a handy option in life; it's a critically important need without which the organism cannot live. Amongst mammals this system is always, without exception, the renal system; it is never, ever, any other

system... period. Other types of vertebrates utilize other systems -- gills, salt glands, or the skin itself -- but mammals always use the kidneys. Period.

This is not opinion or hypothesis, but basic physiological fact.

1988 *Animal Physiology: Mechanisms and Adaptations* (3rd edition) by Roger Eckert (University of California, Los Angeles), with Chapters 13 and 14 by David Randall (University of British Columbia), revised in part by George Augustine (University of Southern California). W.H. Freeman and Company: New York.

pg. 393:

The Vertebrate Kidney

To speak of the vertebrate kidney would be misleading unless we note that it is organized somewhat differently in different groups of vertebrates. Comparisons can be made after we first consider the mammalian kidney. The mammalian kidney performs certain functions that in lower vertebrates are shared by such organs as the skin and bladder of amphibians, the gills of fishes, and the salt glands of birds and reptiles. It is also the osmoregulatory organ of which we have the most complete understanding, thanks to extensive research over the past four to five decades.

pg. 415:

Salt glands have subsequently been described in many species of birds and reptiles, especially those subjected to the osmotic stress of a marine or desert environment. These species include nearly all marine birds, ostriches, the marine iguana, sea snakes, and marine turtles, as well as many terrestrial reptiles. Crocodilians have a similar salt-secreting gland in the tongue.

pg. 415:

The formation of nasal gland fluid does not include filtration, as does the formation of urine by the glomerular kidney.

1978 *Comparative Anatomy of the Vertebrates* (4th edition) by George C. Kent (Department of Zoology and Physiology, Louisiana State University, Baton Rouge, Louisiana). C.V. Mosby Co.: Saint Louis.

pg. 326:

Sweat glands eliminate some salt in mammals, but salt loss by this route is merely incidental to secretion of water for its evaporative cooling effect. It is not a regulated route for salt excretion and, in fact, salt lost by this route usually must be replaced. Mammals eliminate excess salt via kidney tubules.

1990 *Sea Otters* by Marianne Riedmann. Monterey Bay Aquarium: Monterey, California

pg. 35:

Otters and most other marine mammals are constantly in danger of losing water to the more concentrated, or salty, ocean in which they live. They must keep the osmotic concentration of their body fluids constant despite their saltwater environment, a process known as osmoregulation. And besides living in sea water, sea otters eat marine invertebrates that have an elevated salt content, much higher than that of fishes, and often as high as that of the surrounding sea water. This salty diet raises the otter's "salt load" even further. Sea otters conserve water and maintain a suitable water balance by means of their large, heavily lobulated kidneys. Their kidneys are very efficient at concentrating urine by absorbing water and eliminating excess salt in urea, a waste product that's more concentrated than ocean water. Yet exhibit otters living at the aquarium sometimes drink fresh water from a hose and sprinklers that clean the windows. Although wild otters lack sources of fresh water, they do drink sea water, which may help them process and eliminate urea.

1990 *The Pinnipeds: Seals, Sea Lions, and Walruses* by Marianne Riedmann. University of California Press: Berkeley, L.A., and Oxford.

pg. 31:

Pinnipeds conserve water and maintain water balance in several ways. The heavily lobulated kidneys of seals and most marine mammals are extremely efficient at concentrating urine; they absorb water and eliminate excess salt in urine that has a concentration equal to or greater than seawater [various refs that I didn't copy]. The chloride concentration in seal urine is therefore as high as or higher than that of ocean water (which contains an average of 535 millimoles of chloride per liter). A seal that drinks a liter of seawater, after excreting the salts, ends up with a slight net gain of pure water. A human, however, experiences a net water loss of about a third of a liter of water after drinking the same amount of seawater. Human kidneys are less powerful than those of seals or whales, and even the most concentrated urine they can produce is always more dilute than salt water. Water is therefore wasted in eliminating the excess salt in urine.

1984 *Grzimek's Animal Life Encyclopedia* vol 11: Mammal II Dr. Bernhard Grzimek, ed. Van Nostrand Reinhold: New York, Cincinnati, Toronto, London, Melbourne

"Whales" by Dr. Everhard J. Slijper (Professor, Zoological Laboratory, University of Amsterdam) and Dr. Dietrich Heinemann (Director, Zoological Garden, Munster)

pg. 462, 467 (pages 463-466 are plates):

Vertebrate animals are not really built for an aquatic life; the salt content of their blood and body tissues is less than that of sea water. Thus the excess salt which is accumulated from food and water must be eliminated from the body. Whales do not have salt excretory cells like those the bone fishes have on their gills. They are not able to excrete salt through their nasal glands like many sea birds. Whales also do not have perspiration glands, which help many other mammals give off excess salt. The kidneys are the whale's only salt-excreting organs. Whales have very large kidneys relative to those of land mammals; the kidneys of small whales are twice as large as those in land mammals of the same size. In addition, the whale's kidneys are divided into several lobes, the *renculi*, so that the important exterior surface is increased. Whales are probably able to excrete large quantities of urine with their kidneys. We do know that the urine of the bottle-nosed dolphin occasionally contains particularly large amounts of salt.

Salt glands

In the section on tears you'll find that AAT/H proponents claim tears in humans are a method of actively excreting salt in response to a saltwater environment, but in fact, tears are never hypertonic and cannot do what AAT/H proponents claim for them. Tears are, however, strongly hypertonic in regard to potassium and that *does* indicate what environment we developed in. That environment is terrestrial, not aquatic. The salt glands of terrestrial birds and reptiles are specialized for excreting potassium, unlike those of their marine relatives.

Some AAT/H proponents, most notably Elaine Morgan, have insisted that salt glands do not exist in terrestrial birds and reptiles. I must admit I find it discouraging that someone who is making up a theory with which they wish to supplant the past several decades of paleoanthropology does so little research before making such claims. This info is readily available.

Of course, it may be that it isn't simply poor research, since Morgan continued to make the "salt glands only in marine birds and reptiles" claim long after the facts (with refs and accurate quotes) had been presented to her (in several newsgroup posts which she replied to and so presumably read). She did finally say that she was dropping these bogus claims, but these are related to her claims that human tears and sweat are aquatic adaptations, and she and other AAT/H proponents still make those claims. As always, the "false facts" generated by AAT/H proponents are persistent, and so I feel the facts must be stated once again.

1975 *Salt Glands in Birds and Reptiles* by M. Peaker and J.L. Linzell (Dept. of Physiology, Agricultural Research Council, Institute of Animal Physiology, Babraham, Cambridge). Monographs of the Physiological Society No. 32. Cambridge University Press: Cambridge, London, New York, Melbourne.

Mentioned in text:

Some terrestrial birds with salt glands:

1. Ostrich
2. a Partridge, *Ammoperdix heyi* (North Africa and the Middle East deserts)
3. various Falconiformes (birds of prey), including Red-Tailed Hawks
4. North American Roadrunner, *Geococcyx californianus* (particularly in nestlings)

Some terrestrial reptiles with salt glands:

1. Green or Linnean Iguana, *Iguana iguana*
2. Desert Iguana, *Disposaurus dorsalis* (North America)
3. at least one variety of Mastigure, *Uromastyx aegyptius* (North Africa and the

Middle East deserts)

4. Chuckwalla

5. False Iguana, *Ctenosaura pectinata*

pg. 259:

Table 13.3. Salt glands in lizards

Nasal salt secretion observed

Iguanidae *Iguana iguana*

Dipsosaurus dorsalis

Crotaphytus collaris

Sauromalus obesus

Sauromalus hispidus

Sauromalus varius

Sceloporus cyanogenys

Conolophus subcristatus

Amblyrhynchus cristatus (Marine)

Agamidae *Uromastyx aegyptius*

Uromastyx acanthinurus

Scincidae *Eumeces skiltonianus*

Trachydosaurus rugosus

Xantusidae Species not stated

Lacertidae *Acanthodactylus* sp.

Teiidae Species not stated

Varanidae *Varanus gouldii*

Nasal salt secretion not observed

Gekkonidae *Coleonyx* sp.

Agamidae *Amphibolurus ornatus*

Lacertidae *Lacerta viridis*

Anguidae *Gerrhonotus* sp.

Other families with representatives lacking salt glands:

Anniellidae, Teiidae, Cordylidae, Helodermatidae.

pg. 261:

The evolution of salt glands, or indeed of any homeostatic system, is clearly a fascinating subject for speculation. Cranial salt glands are only known to exist in birds and modern reptiles. Therefore it is the reptiles to which we must concentrate most of our attentions. Nasal glands probably first appeared in the Amphibia serving to moisten and cleanse

the nasal passages of the adults, which are usually terrestrial (see Bang & Bang, 1959). Judging by extant species the kidneys of reptiles and birds are much less 'efficient', in terms of concentrating ability, than those of mammals. Therefore evolution of amphibian nasal glands into salt-secreting glands might be inferred to have occurred in the early reptiles since, in truly terrestrial vertebrates, two major avenues for ionic and osmotic regulation present in lower vertebrates would have been lost -- the gills and the skin. In fact the development of the reptilian skin, which is relatively impermeable, from the amphibian type across which ions and water movements occur and can be controlled, must have meant a major alteration in the control of salt and water metabolism. Therefore any animal with an additional means of excreting ions would be at an advantage. It would enable such creatures to live on plants, which are rich in potassium and/or return to the sea and eat plants and invertebrates, which have the same ionic content as sea water. Therefore one might argue that the evolution of a salt gland together with all the other adaptations which enabled the initial colonization of land, was an important means of permitting the vast and complex adaptive radiation that occurred to fill all the varied ecological niches.

Update note: some proponents of the AAT/H have moved away from the idea that the aquatic phase was in a salt water environment, but often still claim humans' sweat and tears systems evolved as an aquatic feature, as an excretory system for salt. This is a remarkable development, and demonstrates one of the reasons the AAT/H isn't taken seriously by those who study the evidence. If tears and sweat in humans are claimed to be due to an aquatic lifestyle and the need to excrete salt, the aquatic lifestyle must have been in a salt water environment. A freshwater aquatic period would be no more or less likely to produce these adaptations than a terrestrial lifestyle, so the purported aquatic lifestyle doesn't explain the supposed adaptation of sweat and tears as systems to excrete salt. Of course the AAT/H proponents are wrong about what the human sweat and tears systems can do, so that just compounds their error.

One such proponent, Algis Kuliukas, has for years repeatedly demanded that I remove this section critiquing the AAT/H claims about salt on the grounds that it has been dropped and was never more than a "tiny little side issue". The reason I haven't removed it is that there are several problems with this characterization. One is, of course, that the issue is still in the many books Elaine Morgan wrote and people still read them and are taught a falsehood that needs to be corrected with accurate information. Another is that the way this information was presented, in direct contradiction to facts available in the references Morgan used in making her false claims, shows how poor the research is that makes up the AAT/H.

And another is that Kuliukas' demand seems disingenuous, seeing as how *he himself* made the salt claims I'm talking about up until this year (2011) and the claims are still being made by a person writing in an upcoming ebook that Kuliukas is coediting. All this even as he called for the critique to be dropped.

The characterization of this as a "tiny little side issue" also doesn't fit with its prominence in Morgan's writings up until she was informed of the facts by me and others in the mid 1990s. It appears over several pages in Morgan's 1972 *Descent of Woman*; gets an entire chapter and part of another in her next book, *The Aquatic Ape* (1982); and another chapter in *The Scars of Evolution* (1990), and was mentioned yet again in her chapter in *Aquatic Ape: Fact or Fiction?* (1991). This does not include the many articles and newsgroup posts Morgan which included these bogus claims. If it was a "tiny little side issue" one is forced to wonder that she wrote about it so much; to say this is minor is to either be deliberately lying or to be ignorant about Morgan's writings.

Salt and the AAT/H

Historically, AAT/H proponents have generally claimed that the transition to hominid took place in a salt water environment, and that this explains several salt-related features which they say are unique to humans: these include salty tears, salty sweat, and not having the same reactions to salt as other terrestrial mammals. AAT/H proponents claim that while other terrestrial mammals exhibit salt hunger and will search for salt, this reaction is not found in humans. AAT/H proponents also say that other terrestrial mammals react to salt depletion just as promptly as they do to thirst, and that they take in only as much salt as they need and no more.

There's one little problem with these "facts": they're all false.

First, tears and sweat *are* salty in *all* animals, because the fluid source of tears and sweat -- the body's plasma -- is salty. However, neither human tears or sweat are ever hypertonic (more salty than plasma), as they would have to be to be a method of salt excretion. (Salt excretion in *all* mammals is handled by the kidneys, which can produce a hypertonic fluid.)

Second, some of the above-claimed reactions to salt depletion are simply untrue, while the ones which are true are shared by humans and other mammals which evolved in a salt-poor environment. Those reactions are missing in mammals which evolved in salt-rich environments. The physiological evidence is clear that humans evolved in a salt-poor environment. Where is this evidence found? In the exhaustive studies by Derek Denton (which, ironically, AAT/H proponent Elaine Morgan has claimed say the exact opposite of what Denton actually does say).

Salt Appetite: What it is; what it isn't

In my exchanges with AAT/H proponents this point has been commonly misunderstood, so I want to state it as clearly as I can:

- Humans -- like sheep, rats, and other non-carnivorous mammals which have been tested -- exhibit instinctive reactions to salt -- salt appetites -- which are common to mammals which evolved in a salt-poor environment.
- "Salt appetite embraces four facets of behaviour: there is hedonic liking for salt unrelated to need, the hunger which follows body sodium deficiency, the hunger engendered by the hormones of the reproductive process, and the appetite evoked by the hormonal response to stress." Derek Denton 1982: 451 *The Hunger for Salt*, Springer-Verlag: Berlin, Heidelberg, New York.
- What salt appetite **is not**: it's *not* a leftover from a previous period when your ancestors had lots of salt, so you got used to it and now crave it.
- What salt appetite **is**: it *is* the legacy of a past wherein your ancestors had little salt, so those who sought it out and responded dramatically to it had an evolutionary advantage.

You may find that reading excerpts from a couple of sci.anthropology.paleo posts helps in understanding these points. They are mistaken statements from posters there (whose identities I've held back to alleviate their embarrassment) and my responses on the subject of [salt appetite](#).

Recap On the Subject of Salt Appetite

All animals need to maintain a balance of water and electrolytes in their bodies, especially critical is the balance of water and salt. *This is not something unusual or unique to a few animals. It's universal.* Mammals which exhibit salt appetites do so because they lived in ways which made it more difficult for them to get enough salt. There was a strong natural selection pressure for them to have this appetite, this hunger for salt. Mammals which evolved getting plenty of salt don't need a salt appetite. They get enough without any need to search or do anything special. These include marine mammals, who live in an extremely salt-rich environment (both their water and their food is salt-rich), and carnivores, who get salt by eating meat.

AAT/H proponents claim that our reactions regarding salt are evidence for the AAT/H, but the "facts" they base this conclusion on are false. The fact that humans -- and our close primate relatives -- have this instinctive trait -- salt appetites -- is

powerful evidence that AAT/H proponents are wrong; the transition from ape ancestor to hominid did not take place in a marine environment.

In pages 100-103 of *The Scars of Evolution* (1990) Elaine Morgan uses Derek Denton's 1982 book, *The Hunger for Salt*, as support for claims that are contradicted by the very book she cites. She also quotes Denton in her endeavor, and this quote is both subtly altered and out of context. Below are examples covering this aspect of what is, unfortunately, typically error-prone AAT/H research. They range from an initial summary up to an in depth (and rather long) selection of apropos quotes. In these you can see how the principle AAT/H proponent, Elaine Morgan, used an first-rate, accurate source as "support" for her claims, when in his book he actually said the exact opposite of what she claimed -- on every point.

Morgan has said she is withdrawing these claims about salt, but there are several reasons these corrections are here. First, she has previously said she had "dropped" evidence yet later picked it up and run with yet again -- in one case the day after she said she'd "dropped" it. Plus, we're dealing with "false facts" and as Darwin said, they "often endure long", so we have to go to the source and point out what has been said regarding salt in support of the AAT/H and where it is wrong. Finally, these corrections demonstrate the caliber of research the AAT/H has been built on, a subject which *should* be of concern even to AAT/H proponents.

Elaine Morgan's AAT/H Research on Salt: How good is it?

This rather long section provides a window on the typically poor quality of research which has gone into building the AAT/H. By providing quotes from Morgan's work as well as quotes from the book she used as the source for her statements, one can see a remarkable lack of accuracy in her AAT/H research. This is important because:

1. She is the best known and widest read advocate of the AAT/H; and
2. Her claims are repeated again and again by AAT/H proponents; therefore
3. It is important (and proper) to examine the accuracy of her research.

In pages 100-103 of *The Scars of Evolution* (1990) Elaine Morgan uses Derek Denton's 1982 book, *The Hunger for Salt*, as support for claims that are contradicted by the very book she cites.

I begin by delineating those claims about salt which Morgan says she got from Derek Denton's book, then provide apropos quotes from both Morgan's and Denton's books showing that a) my delineation of Morgan's claims is accurate, and b) that her claims are contradicted by the very book she uses to support them.

Morgan's claims about salt

These are the claims about salt which Morgan says she got from Derek Denton's book. They center around her contention that humans are fundamentally different from other terrestrial mammals in their physiological reaction, or lack of reaction, to salt:

1. While other mammals have a innate physiological reaction to salt need, humans have no such innate physiological reaction to salt need.
2. Humans do not undertake any compulsory search for salt when deficient, and human intake of salt bears no relation to salt deficit or surplus.
3. Other mammals respond just as urgently to a deficiency in salt as they do to a deficiency in water.
4. >Other mammals take in the precise amount of salt they need to correct their deficiency and then "will take no more".
5. Non-human mammals go to great lengths to satisfy their salt hunger, but humans do not.

Apropos Quotes from Morgan's and Denton's books

This section contains many quotes from Morgan's 1990 book to show that she does in fact make these claims, and from Denton's 1982 book to show that Morgan's claims are not supported by the very book she cites, and quotes, in support of them:

Quotes marked "Morgan", unless otherwise noted, are from:

1990 *The Scars of Evolution* by Elaine Morgan, Souvenir Press: London.

Quotes marked "Denton" are from:

1982 *The Hunger for Salt* by Derek Denton, Springer-Verlag: Berlin, Heidelberg, New York.

Morgan's claim no. 1:

While other mammals have a innate physiological reaction to salt need, humans have no such innate physiological reaction to salt need.

Morgan, pg. 100:

"A salt crisis in our evolutionary history would go far to explain one other specifically human characteristic -- the fact that we have no instinctive awareness of the state of the sodium balance in our bodies."

Morgan, pg. 102:

"An ape living on sea food would have been comparatively well placed to adjust to a saline environment. [2 sentences about sweating deleted] The instinct for responding to 'salt hunger' could well have been lost at this stage."

Denton quote, pg. 605:

"The hedonic human liking or appetite for salt when it is available, and independently of any metabolic need, is an evolutionary legacy of high survival value. It is part of the overall innate organization dedicated to salt ingestion along with the elements determining appetite response to sodium deficiency and to the hormones of the reproductive process."

Denton quote, pg. 605:

"Further, learning or habituation may be predominant in the gradual augmentation of intake over years. Indeed, even in a herbivorous creature such as the sheep, whose highly developed appetite organization has been extensively investigated, it is obvious that learning mechanisms become richly superimposed on innately generated drives. This, of course, is characteristic of innate mechanisms in that most can be modified by learning.

"But in the human case, the cultural influences are acting in the context of an innate propensity: the liking for the taste of salt and readiness to ingest it."

Morgan's claim no. 2:

Humans do not undertake any compulsory search for salt when deficient, and human intake of salt bears no relation to salt deficit or surplus.

Morgan, pg. 101:

"In humans neither the compulsory search nor the abrupt cut-off point can be relied on. Their intake bears no relation to salt deficit or surplus."

Denton quote, pg. 117:

"A searching example of the 'set point' of sodium homeostasis has been made by Hollenberg (1980). He emphasizes that sodium homeostasis, especially control of extracellular volume, is an excellent example of a feedback control system. The afferent and efferent limbs of the system have received considerable attention but the 'set point' has received remarkably little emphasis. He notes a personal clinical

observation of a normal man, in sodium balance on a 10 mmol/day intake, inadvertently being given a 30 mmol sodium load intravenously and promptly excreting it."

Denton quote, pg. 117:

"Similar findings have been documented in several studies (Braunwald et al. 1965; Hollenberg et al. 1972), including the outstanding work of Strauss and colleagues (1958). They showed that a person in balance on a low-salt diet responded with a prompt natriuresis to a 30 mmol sodium load. If a diuretic was administered beforehand and 100 mmol of sodium lost, then the 30 mmol load did not cause natriuresis. Indeed this did not occur until the 100 mmol lost with the diuretic was replaced. The data suggest the 'set point' around which sodium balance cycles in the normal person is that amount of sodium chloride in the body when the person is in balance on no-salt intake (Hollenberg 1980)."

Denton quote, pg. 180:

"Yensen (1959) showed that salt deprivation in two normal humans reduced the taste threshold for salt to approximately 1 mmol/l, but did not alter the threshold for sour, sweet or bitter substances. No salt craving developed, but at meals the subjects felt the food would taste better with salt. Forced drinking of water, which resulted in negative sodium balance, lowered taste threshold and this was not associated with lowered salivary Na/K ratio (de Wardener and Herxheimer 1957). An intense craving for salt developed in the subjects."

Morgan's claim no. 3:

Other mammals respond just as urgently to a deficiency in salt as they do to a deficiency in water.

Morgan, pg. 100:

"When we are suffering from a deficiency of water we feel thirsty, and take active measures to find a source of water and set the balance right. If the thirst remains unslaked it intensifies until all other considerations become subordinate to the need to drink.

"Most mammals respond just as urgently to the need for salt if they are deprived of it."

Denton quote, pg. 221:

"A characteristic feature of salt appetite is the delay in its appearance despite large rapid loss of sodium. With sheep the delay is usually 24 h, with rats 4-8 h. This is in striking contrast to thirst."

Denton quote, pg. 223 (concerning sheep):

"The results of experiment (i) on five animals were clear-cut. As Fig. 12-1 (results on four animals) shows, any significant increase in voluntary drinking of sodium was delayed 2-4 days, by which time there was severe deficit."

Denton quote, pp. 223-224 (concerning sheep):

"Whereas sodium intake was delayed, water intake usually rose on the first or second day (Abraham et al. 1976)."

Denton quote, pg. 224 (concerning sheep):

"Overall, however, the data confirm the delayed onset of appetite despite very rapid large loss of salt."

"Other studies, with other methods, also indicate this delayed onset. The contrast with thirst is striking."

Denton quote, pg. 458:

"The signal or outstanding feature of salt appetite, apart from its being innate, is the time delay in its onset. It follows 4-72 h after establishment of body deficit in the naive animal, be that rat, sheep or rabbit."

Denton quote, pg. 458:

"The character of the behaviour contrasts with thirst. Though the two ingestive behaviours have common elements in the operation of taste factors and oropharyngeal metering of inflow associated with satiation of appetite, the excitation of them is quite different. With thirst a sudden rise in the [Na] (sodium concentration) of cerebral arterial blood contrived by intracarotid infusion, or a rise in angiotensin concentration produced similarly, results in avid drinking within 30-120 s. Similarly with isosmotic volume depletion contrived by sequestration with formalin, polyethylene glycol or haemodialysis, the onset of thirst is rapid. The reaction of the osmoreceptors, sodium receptors, and possibly angiotensin II receptors as well as those transmitting from the left atrium via the vagus is rapid, the redirection of the stream of consciousness as a result of reticular arousal and cortical reaction is rapid, and the seeking and drinking of water is more or less immediate according to circumstances."

Morgan's claim no. 4:

Other mammals take in the precise amount of salt they need to correct their deficiency and then "will take no more".

Morgan, pg. 100:

"Derek Denton, in his classic study *The Hunger for Salt*, describes his researches

into salt appetite in non-human mammals such as sheep, rats and rabbits. In all these species there is a precise correlation between the amount of salt their bodies need and the amount they will take in."

Morgan, pg. 101:

"On the other hand, when an animal has had enough salt it will take no more."

Denton quote, pg. 221:

"In rats with sodium deficit as a result of adrenal insufficiency, the characteristic behaviour was to overdrink [a saline solution] considerably relative to deficit."

Denton quote, pg. 549:

"Within a particular species, such as the sheep, there may be large individual variation in the 'set' of the hedonic or palatability mechanisms determining 'need-free' intake of salt. Given the same environment and dietetic conditions from birth, and these, for example, involving low but adequate salt intake in food and water, some animals when given free access to sodium solutions behave as salt gluttons, whereas others ingest little or no salt."

Denton quote, pp. 184-185:

"In relation to the characteristics of salt ingestion by rats on a diet with adequate sodium content, Richter (1956) has represented the parameters of the behaviour as shown in Fig. 10-13. A typical set of observations is shown in Fig. 10-8. Similar types of preference behaviour are shown towards other substances, for example sucrose and alcohol. Clearly this does not reflect any bodily deficit but a liking for the substance (Young 1948).

"Young (1948) has pointed out that in terms of the theory that intake indicates bodily need, one would expect the average daily intake of a substance to be approximately constant under the same set of intraorganic conditions. However, in the normal rat the quantity of fluid ingested varies markedly with concentration. Weiner and Stellar (1951) have also pointed out that the ingestion of some constant amount of salt is not the goal of animals studied in relation to salt preference. Their experimental technique also revealed that the rats drink the preferred concentration much more rapidly."

Denton quote, pg. 209:

"Summary of sheep experiments"

"These experiments had certain clear results.

1. "In all animals development of sodium deficiency caused a large voluntary intake of sodium solutions. Onset of the appetite, whether indicated by clear change of behaviour or intake of solution, was usually delayed 2-5 days.

2. "Sodium intake was eventually adequate to maintain the animals in good condition physiologically. There was variation, however, in whether the sodium status was maintained on the positive side with a sizeable amount of sodium in the urine, or whether near neutral or slightly on the negative side."

Denton quote, pg. 211 (concerning sheep):

"However, despite the naive animal sometimes approximating intake to the extent of its deficit from first instances of sodium deficiency, it is obvious that with many animals a large element of experience and learning antecedes such behaviour."

Note: While we can see from the Denton quotes above that non-human mammals do not generally have an "abrupt cut-off point" (as Morgan claims) when it comes to salt intake, note that rabbits are an exception (and a "striking" and "remarkable" exception at that) to this general rule:

Denton quote, pg. 240:

"Rabbits"

"The intriguing data on the capacity of adrenalectomized rabbits to repair body deficit precisely without excess intake are dealt with in Chapter 14 bearing upon satiation behaviour."

Denton quote, Chapter 14, pg. 255:

"Wild rabbits, sodium-deficient as a result of adrenal insufficiency, take 9-12 h to correct deficit whether large or small. That is, the rate of drinking is determined by the amount of deficit. The precise correlation between deficit and intake is striking."

Denton quote, pg. 258:

"Rabbits"

"The results of studies by Dr J. Nelson on the time course of satiation of sodium deficiency by wild rabbits have been rather remarkable."

Denton quote, pp. 258-260 (concerning rabbits [pg. 259 is figures]):

"Fig. 14-4 shows the time course of correction of sodium deficit. The deficit was exactly replaced by 12 h. The animals were still in exact balance at 24 h, indicating there was no overshoot as with rats. The striking feature was that the same linear correlation between intake and initial deficit was seen at 2, 4, 6 and 9 h (Fig. 14-5) and the correlations at all times were highly significant ($P < 0.01$ at all stages). Whereas the animals were slow to repair deficit, the rate of drinking was always related to the degree of the initial deficit, the more deplete animals drinking faster over the 12 h. If the animal were less deplete it drank slower though the rate of drinking with large deficit showed it could have repaired the small deficit in much

less than 9-12 h. Additional to the observation on no overshoot noted above, the rabbits were also in balance at 48 h."

Morgan's claim no. 5:

Non-human mammals go to great lengths to satisfy their salt hunger, but humans do not.

Morgan, pg. 101:

"In humans neither the compulsory search nor the abrupt cut-off point can be relied on."

Morgan, pg. 100:

"Species living in habitats far from the sea go to great lengths to satisfy their salt hunger."

Denton quote, pg. 89:

"There is an emphasis in relevant historical records on a human preoccupation with salt. Sometimes this may amount to a craving and people may endure great hardships and take risks to obtain it."

Note: Morgan followed her above sentence with two paragraphs of examples of mammals' search for salt which were drawn from Denton's book, but somehow managed in them to skip all his mentions of humans. She also must have entirely skipped Denton's Chapter 5, "Salt in History: Symbolic, social and physiological aspects", which consists of relevant data contradicting several of her points. Just a coincidence?

Summary of Elaine Morgan's AAT/H Research on Salt: How good is it?

This section examines Morgan's AAT/H claims about salt and its relevance to human evolution, which she says were based on Derek Denton's excellent research on salt and salt appetites in a variety of mammals. It demonstrates that Morgan's claims are contradicted by the very work she based them on.

The term "salt appetite" or "salt hunger" is described by Denton as encompassing several aspects of behavior with an instinctual basis:

Denton, Chapter 24, pg. 451:

"Salt appetite embraces four facets of behaviour: there is hedonic liking for salt

unrelated to need, the hunger which follows body sodium deficiency, the hunger engendered by the hormones of the reproductive process, and the appetite evoked by the hormonal response to stress."

While Morgan claims specifically that in humans the "instinct for responding to 'salt hunger'" has "been lost", Denton's book demonstrates that in humans as in other non-carnivorous mammals there is an innate, powerful salt appetite, which entails an "overall innate organization dedicated to salt ingestion along with the elements determining appetite response to sodium deficiency and to the hormones of the reproductive process."

Indeed, the central theme in his book is that there is an innate physiological basis for salt appetite shared by humans and other non-carnivorous mammals. Since Morgan used it as a reference, and since this conclusion is stated both at the beginning and end of Derek Denton's book, one wonders how she managed to miss it.

Denton, pg. 1:

"The important role of salt in the history of civilization will be recounted. The thesis is that this is no accident when considered against the metabolic phylogeny of feral man and the selection pressures which must have operated -- particularly during the reproductive process."

Denton further states that humans share with non-human mammals the characteristic that "learning mechanisms become richly superimposed on innately generated drives. This, of course, is characteristic of innate mechanisms in that most can be modified by learning." And he emphasizes that "in the human case, the cultural influences are acting in the context of an innate propensity: the liking for the taste of salt and readiness to ingest it."

That Morgan could miss this clearly stated thesis bespeaks either an incredible blindness to anything not supporting her claims, or a suspiciously selective approach to source material to a degree usually seen only in the writings of creationists. Perhaps she sees data much as Philip Kitcher describes Creation "scientists" doing: "For Creation 'scientists' data has only one function; it is a potential source of problems for evolution." (Kitcher, 1982:133).

Morgan claims that with humans, "their intake bears no relation to salt deficit or surplus."

Denton, on the other hand, describes sodium homeostasis in humans as in other mammals as "an excellent example of a feedback control system". He describes studies showing that humans with salt deficit develop "an intense craving for salt",

and gives examples of several studies of humans' reactions to salt overload, such as: "being given a 30 mmol sodium load intravenously and promptly excreting it."

Morgan's claim that "in humans neither the compulsory search nor the abrupt cut-off point can be relied on" is a classic of misdirection; it's connected to her bogus claim that in "sheep, rats and rabbits" there is "a precise correlation between the amount of salt their bodies need and the amount they will take in" and that "when an animal has had enough salt it will take no more." Part and parcel with this statement is her claim that non-human mammals "respond just as urgently to the need for salt if they are deprived of it" as they do to the need for water.

Although she explicitly cites Denton as the source of these claims, Denton actually repeatedly points out that "the signal or outstanding feature of salt appetite, apart from its being innate, is the time delay in its onset" and that this "is in striking contrast to thirst." He gives clear-cut results from tests on sheep showing that "any significant increase in voluntary drinking of sodium was delayed 2-4 days, by which time there was severe deficit." He repeats that "other studies, with other methods, also indicate this delayed onset. The contrast with thirst is striking."

Denton points out that although salt appetite and thirst "have common elements in the operation of taste factors and oropharyngeal metering of inflow associated with satiation of appetite, the excitation of them is quite different." He shows that although salt hunger has a characteristic delay in onset, "the seeking and drinking of water is more or less immediate according to circumstances."

While Morgan claims that "when an animal has had enough salt it will take no more", Denton shows that in rats, for example, "the characteristic behaviour was to overdrink considerably relative to deficit." He also shows that there is considerable variation within a species, and that whereas, in the same circumstances some non-human animals "ingest little or no salt", others "behave as salt gluttons". "Will take no more" indeed! Hardly a characteristic of a "salt glutton".

He points out that the characteristic behavior of rats to salt clearly "does not reflect any bodily deficit but a liking for the substance" despite Morgan's claims to the contrary.

He also shows that there is a large amount of learning connected with this innate behavior, and that these animals' instinct does not, as Morgan claims, result in "a precise correlation between the amount of salt their bodies need and the amount they will take in". Denton actually says that despite the animal "sometimes approximating intake to the extent of its deficit", "it is obvious that with many animals a large element of experience and learning antecedes such behaviour."

So contrary to Morgan's claims, it is obvious that non-human mammals do not generally have an "abrupt cut-off point" when it comes to salt intake. Note again that the rabbit is an exception to this general rule, and note how many times Denton points out the (and I quote) "remarkable" nature of this exception.

Denton is obviously impressed by the unusual ability of rabbits to do what Morgan (incorrectly) claims is a common feature in mammals. He refers to "the intriguing data on the capacity of adrenalectomized rabbits to repair body deficit precisely without excess intake" and says "the precise correlation between deficit and intake is striking." In other places he says this ability is "rather remarkable" and calls it a "striking feature". If it were a commonplace ability, as Morgan claims, one wonders why on earth Denton is so darned impressed. He's impressed because it's not the common ability Morgan says it is.

Morgan points out that many non-human mammals "go to great lengths to satisfy their salt hunger" and illustrates this point with examples drawn from Denton's book, but shies from citing his mentions of humans also going to great lengths to do the same. As Denton points out, humans take this to the point of warfare, and he adds that "there is an emphasis in relevant historical records on a human preoccupation with salt. Sometimes this may amount to a craving and people may endure great hardships and take risks to obtain it."

To say the least, Morgan is selective in her use of Derek Denton's work. It would even seem, from the fact that she claims he says things when he in fact says the opposite, that she either has not done careful reading, or has deliberately misrepresented his work.

Lastly, toward the end of her section on salt and its relationship to environment during human evolution, Morgan attempts to convince us that we are marine creatures at heart by telling us a sad story about children in famines, and how their death rates were halved by oral rehydration therapy:

Morgan, pg. 102: "Oral rehydration therapy -- a simple cheap solution of sugar and salt -- instantly halved the diarrhoea death rate in the villages where it was administered."

So now, unless this whole section of her book was simply an elaborate non sequitur, a completely off-the-subject digression, Morgan now apparently also proposes that in addition to a few million years spent in a saltwater environment, we must also have spent a similar period of immersion in sugar-water!

References

1982 *Abusing Science: The Case Against Creationism*, by Philip Kitcher. The MIT Press: Cambridge, Mass. and London, England.

1990 *The Scars of Evolution*, by Elaine Morgan. Souvenir Press: London.

1982 *The Hunger for Salt*, by Derek Denton. Springer-Verlag: Berlin, Heidelberg, New York.

Mating Postures and the AAT/H

That many aquatic mammals copulate facing each other is due to constraints imposed by their anatomy. Tails which have become flukes, for instance, simply get in the way. Moreover, not all aquatic mammals copulate facing each other, despite the fact that AAT/H proponents claim -- or at least *very strongly imply*-- they do. In some cases -- sea otters, for example -- observers are actually surprised that they *don't* do so ("it would seem simpler," one said), but they don't, despite their mating in the water. In fact, only those aquatic mammals which, due to anatomy, are unable to mate any other way, copulate front to front, and these are all animals which have been aquatic for tens of millions of years (ranging from 20-50 million years). This is several times longer than the entire existence of hominids, from our divergence from apes to the present day.

Further, there are terrestrial mammals which commonly copulate facing each other, and they are close to home. Two of our close primate relatives, orangutans and pygmy chimps (bonobos) do so regularly, as well as black-handed spider monkeys; gorillas do so occasionally, as do woolly spider monkeys. (My thanks to Steve Foster for the info about gorillas and the spider monkeys -- see his web site link on my Links page.)

And lastly, does it really have to be pointed out that humans don't always copulate facing each other?

Since this sexual position is not uncommon among higher primates, it shouldn't be surprising to find humans doing it. Why does this surprise AAT/H proponents so? I don't know.

Body Temperature and the AAT/H: Does the human condition indicate an aquatic past?

AAT/H proponents often make several claims about mammalian body temperatures:

1. The body temperature of normal, healthy humans doesn't fluctuate, while that of terrestrial mammals does.
2. Normal human body temperature is like that of whales, rather than our primate relatives or other terrestrial mammals.

What we find when we look at the facts (which are readily available) is that the body temperature of normal, healthy humans certainly does fluctuate. In fact, it does so on a regular basis even without exercise. With exercise it fluctuates even more. It is very similar to our primate relatives, even those -- like baboons -- which live in open savannahs. I've got some facts and figures below about body temperatures of several types of mammals.

This leads to a fascinating question: why are AAT/H proponents so often surprised that we are like our primate relatives? For instance, some terrestrial mammals, such as antelope, can allow their body temperature to rise while the temperature of their head (and brains) stays lower. They can manage this because they have a network of blood vessels which flow around the nose so the blood to the brain is cooled by the mouth and nostrils. Humans and other primates, even those which live on open savannahs, cannot do this to any great degree. Also, some terrestrial mammals -- such as horses, cows, goats, and rabbits -- generally have higher body temperatures than humans. AAT/H proponents consider these facts to be evidence that we did not evolve on land; they typically state that we would have a thermoregulatory system just like that of distantly related animals like antelope rather than the type of system seen in other primates. Yet our human system is shared with our primate relatives -- **even those which live on open savannahs** -- so it is *exactly* what we should expect if we evolved as terrestrial mammals.

Lastly, our body temperature is not like that of whales, although all mammals' body temperatures are relatively close ("normals" being between about 34-40 degrees C).

So this is the AAT/H question regarding our thermoregulation (this question was actually asked of me online):

If we were on the savanna for the millions of years as you say, and did not have an intervening period of "aquatic-ness", instead evolving with the other savanna creatures, why then did we not end up evolving the same way?"

First, note that I'm not talking about our earliest hominid features necessarily arising on savannahs. Second, why do AAT/H proponents generally insist that all savannah animals deal with heat in the same way, when in fact they utilize a variety of approaches to this problem? Baboons and patas monkeys, for instance, use a far different system than antelope just like humans do.

Third, as a prelude to answering this, let me present another question along the same lines. Sea gulls and many other sea birds excrete salt through nasal glands; crocodiles do so through salt glands on their tongues (although it should be noted that some scientists believe that these glands were developed to counteract the effects of dehydration, a problem which crocodile, paradoxically, are susceptible to). Why don't they both use the same method? Why don't whales do it the same way? (Instead they use the usual mammalian solution of the kidneys plus hormonal regulation of urine formation and concentration.) They're all in essentially the same environment (marine) and are dealing with the same problem (too much salt). Why do they use very different methods to do the same thing?

The answer is the essence of evolutionary theory: relatedness. Evolution is not like catalog shopping. You can't just "pick and choose" from the Big Book O' Adaptations: I'll have one of those, and, oh, that looks good! just doesn't work in the real physical world. You have to start with what you have, and consequently we shouldn't be surprised to find that we resemble primates in our body temperature and thermoregulatory needs. We've been separated from other animals for tens of millions of years, and just because they can let their temperatures rise by 10 degrees on a hot day doesn't mean we can forsake our primate heritage just because it seems like a handy thing to be able to do. Just as we see with the problem of dealing with too much salt, we see that not all savannah animals deal with heat in the same way. So why should we be expected to be different from other primates, even savannah-dwelling primates, and instead be like distantly related animals like antelope and warthogs, when in fact in evolution distantly related animals generally utilize a variety of approaches to any given problem?

Back to our questioner:

We ended up taking a different evolutionary path from the majority of animals there, now the question is why?

There aren't primates, for one, and more specifically, they aren't apes. We've been separated from even baboons for probably about 20-25 million years. We are far more closely related to apes, and especially to African apes, than to anything else on the planet. That we resemble them and not antelope, cattle, aardvarks, and warthogs is not something to be *wondered at*; it is to be *expected*.

I'm not saying that we first evolved our hominid characteristics on savannahs, although it is obvious that we we're able to live there, as other primates, chimpanzees for example, also can. I am pointing out that it is silly to think that we would evolve the same thermoregulatory system as antelope instead of using similar systems to what we see in other primates, including savannah-dwelling primates. And that in regard to our body temperature we resemble other primates,

including savannah-dwelling primates, unlike the oft-repeated claims of AAT/H proponents.

Why do people harbor the incorrect idea that human body temperature doesn't fluctuate?

Anthony Smith, in his 1985 book, *The Body*, provides an answer:

"The word normal, as applied to human body temperature, is a splendidly misleading description. The little arrow on the clinical thermometer, doggedly directed at 98.4 degrees F (36.9 degrees C), gives extra support to it and the exactness of that point four of a decimal emphasizes it even further". (Smith 1985: 365-366)

[My note: Great Britain uses "98.4 degrees F" as "normal", while the French and North Americans use "98.6 F (37 C)".]

Smith continues:

"Normally the majority of us do not have a 'normal' temperature. Infants have a higher temperature; older people have a lower one. Ordinary adults wake up with their temperature below normal and go to sleep with it above normal." (Smith 1985: 366)

He then elaborates:

"To confound the picture of 'normality' still further there is the daily cycle. Babies acquire it quickly, and then follow the traditional pattern of a peak in the evening and a trough at dawn. An average between maximum and minimum is 2 degrees F (1 degree C), but individuals can go up and down by 3 degrees F (1.5 degrees C) or more. The menstruation of women is a further complexity; during it the temperature is low. This increases slightly with a small but pronounced rise (perhaps as much as a degree) during ovulation when an ovary releases an egg. A hot bath, reasonably enough, send the bather's temperature up to 100 degrees F (38 degrees C) or so. Exercise does even better. After three miles a runner can read 105 degrees F (40.5 degrees C) rectally, but less than normal on his skin due to all the sweating there". (Smith 1985: 368)

So the idea a "normal" human body temperature is an exact number like "98.6" is oversimplified quite a bit; in fact, *Black's Medical Encyclopedia* even lists humans' "normal" body temperature as being between 36.7 and 37.2 degrees C (98-99 degrees F).

Human body temperature compared to primates

So we can see that the body temperatures of normal, healthy humans do vary, contrary to AAT/H claims. We can also see that, contrary to AAT/H claims, our temperatures are like those of our relatives, including open country baboons (*Papio*, listed below):

Macaca mulatta (Rhesus macaque) 36-40 degrees C

Macaca fascicularis (Crab-eating macaque) 37-40 degrees C

Papio hamadryas (Hamadryas baboon) 36-39 degrees C

(From 1987 *The Care and Management of Laboratory Animals* Trevor Poole, ed. Longman Scientific and Technical: Harlow, Essex)

Also note that these temperature ranges are likely slightly higher than those of wild, resting individuals, since in order to take their temperature, these primates must be forcibly restrained:

"In most cases, they are likely to represent normal ranges, but normals are difficult to establish for animals which readily become excited when restrained". (Poole 1987: 602)

Human body temperature compared to whales

As for the supposed similarity in body temperatures between humans and whales, let's just look at what an expert on cetaceans has to say about it:

From 1979 *Whales* (first pub 1958; revised 1962), by Dr. Everhard J. Slijper (Professor, Zoological Laboratory, University of Amsterdam). Hutchinson of London: London.

pg. 301 (after giving the body temperatures from many studies of whales): "For the time being, at least, we may take it that the average body temperature of Cetaceans in general is about 95.9 degrees F. -- a very low figure indeed for a mammal".

"This figure is 2.5 degrees F. below that of man, whose temperature is low in turn when compared with that of horse (100.4 degrees F.), of cows and guinea-pigs (101.3 degrees F.), of rabbits, sheep and cats (102.2 degrees F.), and of goats (103.1 degrees F.). Only hedgehogs are known to have an average summer temperature equal to that of cetaceans, while sloths, opossums, and duck-bills

(89.6 degrees F.-93.2 degrees F.) are even more cold-blooded. But then the last-named species occupy such a special position among mammals in so many respects, that we may say that compared with terrestrial mammals, whales have a very low temperature. Seals and related species certainly have higher temperatures, for Clarke has measured 98 degrees F. in an elephant seal. The hippopotamus, on the other hand, has a temperature similar to cetaceans (96 degrees F.), and sea-cows probably have a lower temperature still".

A few other mammalian body temperatures

From 1991 *Environmental and Metabolic Animal Physiology* 4th edition. C. Ladd Prosser, ed. Wiley-Liss: New York. pg. 111 (from Table 1):

Man 37 degrees C
Baboon 38.1 degrees C
Mountain sheep 37.9 degrees C night
Mountain sheep 39.8 degrees C
Goat 37-40 degrees C
Fur seal 38 degrees C
Humpback whale 36 degrees C
Bat (*Dobsonia*) 37 degrees C

Did you know our body temperature is similar to that of at least some bats?

I didn't.

Does this mean we used to fly?

No.

The "Swimming Babies" reference

You might wonder, as one thoughtful participant in the online AAT/H debates did, why do so many babies drown in bathtubs and swimming pools each year? For AAT/H proponents tell us, as evidence for their theory, that human babies naturally hold their breath and swim.

This is a hoary chestnut indeed which has been used for quite some time by many AAT/H proponents (including Hardy, Morgan, and Verhaegen). This time they usually actually give the reference: "M. McGraw, Journal of Pediatrics, 1939:485-490.", so one might assume they've read this research paper and know what it says. They always seem to mention the human infants and how their movements are usually "rhythmical and organized" and are "ordinarily sufficiently forceful to propel the baby a short distance through the water". So far so good. But

they don't seem to ever mention the fact that the same study looked at other mammalian infants (opossum, rat, kitten, rabbit, guinea pig, and rhesus monkey) and found that they behaved the same way: "these rhythmical movements of the human infant are quite similar to those of other young quadrupeds in water".

AAT/H proponents consistently report *only* the info about human infants, and state that they react in a *unique* manner, ignoring the contrary facts the study reports regarding non-human infants, even though the info about both human and non-human infants is reported *on the same page*. Coincidence?

Note that an older chimpanzee was also tested and, just like older human infants, was inactive when placed in the water. Note too that this study found that "at no time did any baby show himself capable of raising his head above the water level for the purpose of breathing". So that's the sad secret of the drowning infants; what we actually find here is not so much "swimming babies" as infant mammals slowly drowning without a struggle.

Let me take this lead-in to make another point about the supposedly instinctive ability of humans to swim:

It's been a common, even central, claim from various versions of the AAT/H that humans have some special, inbred, and unusual-for-primates knack for swimming. But they don't. In fact, ironically, it seems likely that swimming is one area where -- for similar reasons due to their locomotion specializations -- humans, chimps and bonobos (probably before they diverged from each other), and gorillas all independently evolved a similar trait: the relative inability to swim in an easy, natural, way that allows the animal to swim instinctively. So the 3 groups all independently lost the ability to swim instinctively. In each group it's due to their primary locomotion, knucklewalking for chimps, bonobos, and gorillas, and bipedalism for humans. The knucklewalking adaptation seems to be even worse for swimming than the bipedalism adaptation. It makes one tend to dip down headfirst, which is disastrous unless you learn to overcome it (which according to observers some few -- apparently some *very* few -- gorillas and chimps have managed to do), while the bipedalism adaptation makes you tend to tip back, which also helps you drown but can be overcome somewhat more easily. But both are worse by far than other quadrupedal methods, from what the swimming abilities of other primates and other mammals shows us, and contribute to the high drowning rates seen in humans, including infants, children, and adults. They make the instinctive swimming motions seen in mammals, and the ability to raise the head to breath, counterproductive.

This has been ignored by AAT/H proponents, especially hypocritical of them due to the fact that this has been handily described by Jan Wind in the volume *The Aquatic*

Ape: Fact or Fiction?, a volume those same proponents regularly insist others read and not ignore. Wind also made a point about the ubiquity of instinctive swimming ability in most mammals, including primates, and reported on a test where he placed a tree shrew and a macaque, neither of which had any swimming experience, in water and they both immediately swam perfectly well, quite unlike the typical human experience.

There seem to be few mammals which definitely cannot swim, or which cannot swim instinctively, although many have been erroneously said to be unable (for instance I've seen it claimed erroneously that rabbits, bats, sloths, and kangaroos are unable to do so). And of course there are many mammals which do not ordinarily try swimming, which makes it hard to say whether or not they could. As far as I have seen, humans and the African apes are among the very few which are known to not swim instinctively; two which cannot swim are (as far as I've seen) giraffes (they wade well enough, as you'd expect for an animal which has little trouble keeping its head out of water) and -- ironically enough -- adult hippos, which walk on the bottom but apparently cannot swim (juveniles can). The interesting thing about all of these -- adult hippos, giraffes, African ape, and humans -- is that their shared difficulties are due to mechanics -- weight and denseness in hippos and imbalance due to physical specializations in giraffes, African apes, and humans.

What about predators?

No matter where the transition is said to have taken place, any attempt to construct a theory about early human evolution *must* contend with, among other things, the problem of predation and how to avoid it (as much as possible). When dealing with the idea of the transition from hominoid to hominid on land, we can examine how our close relatives, chimpanzees for instance, deal with potential predators. They are similar in size and intelligence to our ancestors, and how they handle predators tells us something about how our earliest ancestors could handle predators on land.

When dealing with predation during a transitional period in the water, we also have to look at the problem squarely. The several variations of the Aquatic Ape Theory vary in whether the transition happened in salt or fresh water (although fresh water scenarios eliminate any possibility of explaining salt excretion as the salt water scenarios attempt to do), but they all agree that the transition to habitual bipedalism required a water environment. Except for Hardy's original claims, the amount of time actually spent in the water doesn't get discussed much, but we would have to assume that it must have been most of the time spent in locomotion, or we wouldn't expect bipedalism to be adopted for the reasons the AAT/H suggests. The reasons given are that if the hominids weren't bipedal, the water

would be over their heads, and that the water would support the weight of the hominid, lessening strain. Both these requirements of the theory suggest that we're talking about spending many hours a day in water that is considerably more than waist deep. This makes for an enormous amount of exposure to common and vicious predators that would be seldom, if ever, encountered by a land-based transitional hominid (although they would be expected to occasionally get water from water holes, lakes, rivers, and streams, these land-based hominids might reasonably be expected to more often get their water the way chimpanzees in areas without standing water most often do, for instance by using leaves as a sponge).

Most AAT/H write-ups also make statements about the lure of this water environment for safety from predators. However, rather than being a haven from predators, this environment would not only expose such hominids to a great number of predators, but also predators which seem much harder to defend oneself from. To explain this, I'll give information about both sharks and crocodiles, which would be expected to be the main predators of such hominids, whether in salt or fresh water. Both crocodiles and sharks which are highly dangerous to humans even today are to be found in both fresh and salt water in Africa.

The bottom line on dealing with predators is that a species doesn't have to be able to avoid them completely, but in order for the species to survive, they have to avoid predators well enough to be able to replace their numbers. Some animals do this simply by having enormous numbers of young; even some mammals have large numbers of young. But humans and apes don't do this; in fact primates in general have relatively few young. To see if such an animal -- medium-sized and having relatively few young -- can survive predation in a given environment, we can look to see if any such animal actually has managed to do so. For a postulated land-based transition, we do see such an animal -- chimps. For a postulated aquatic-based transition, we see *no such animal of any species anywhere in the world*.

First, do AAT/H proponents claim that an aquatic environment was safer than land?

Sir Alister Hardy, "Was there a *Homo aquaticus*?", article originally appeared in *Zenith*, 1977, vol. 15(1):4-6. Reprinted in 1982 *The Aquatic Ape* by Elaine Morgan, Stein and Day: Briarcliff Manor, N.Y.

pg. 150:

"So after some twenty million years or more of living a semi-aquatic life -- I must make it clear that I do not suppose man spent more than perhaps five or six hours

in the water at a time -- *Homo aquaticus* left the sea (or lake) a very different creature from when he first entered it."

pg. 150:

"Perhaps it was not only a shortage of food that sent man to the water in the first place, but also a means of escaping from powerful predators: possibly *Homo aquaticus* was only able to survive and evolve with the help of a number of small sandy or rocky islands stretching up the tropical coasts or margins of lakes where he could live in large colonies, like those of seals or penguins, and where his only enemies were sharks and killer whales in the sea or crocodiles in lakes and rivers."

The following are from:

1991 *The Aquatic Ape: Fact or Fiction?* Edited by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds. Souvenir Press: London.

Leon P. Lumiere

Chapter 3. "The Evolution of Genus *Homo*: Where It Happened"

pg. 27:

"The dwindling forest would produce exactly the environmental conditions required by the Hardy hypothesis; those apes near the coast, losing their forest, gradually would be forced into water to find both food and protection from predators."

Derek Ellis

Chapter 4. "Is an Aquatic Ape Viable in Terms of Marine Ecology and Primate Behavior?"

pg. 37:

"How did apes survive on the savannah when there were fierce, fast predators there, day and night?"

pg. 67:

"Escape from land-based predators, when the apes were on shore, would be by running back to water and swimming away."

From these quotes you can see that escape from predators is considered to be an important part of the purported aquatic transitional environment. Note that even without considering aquatic predators, the idea of hominids escaping land-based

predators such as lions by "running back to water and swimming away" is problematic, since large cats such as lions often hunt by water holes, streams, etc. and chase their prey into the water and kill them there. Since human swimming speed is much slower than human running speed, the usefulness of this method of escape seems to be an assumption that doesn't hold water. But here I am primarily concerned with describing the habitats and habits of water-based predators, to explain why they are such an intractable problem for the AAT/H, but not for theories of a land-based divergence.

Land-based predators

How chimpanzees react to predators

In reading what AAT/H proponents say -- both in published works and what they say online -- it seems they believe that our earliest hominid ancestors just couldn't have survived on land because of land-based predators. They also seem to feel (because that's what they say over and over) that the only option available to hominids on land is to climb trees or run away from predators. They would've realized they were wrong if they had done even the most basic of research into how our ape relatives handle the dangers of predators.

In fact, predation is not much of a problem overall for chimps, which might seem odd until you look at what you face when you face a chimp. What you face is usually not just a chimp, but a group of chimps. A group of strong, howling, stick and/or rock throwing gang of vicious little hominoids. They kill baboons and leopard cubs (with leopard-mommy present) with no more armament or natural ability than australopithecines had. They are not defenseless out there, and neither were australopithecines. In fact, even lone chimps have been seen sleeping overnight on the ground in areas frequented by leopards, which further suggests that they don't have much trouble with such predators.

What we see when we look at predation on chimpanzees, those animals so similar in size and intelligence to our early ancestors, is that they have more severe problems (ie. they get killed) with leopards in more heavily forested areas. In more open wooded savannah they show little concern with leopards. Perhaps Adrian Kortlandt's experiments during the 1960s would give us a clue as to why this is:

During the 1960s Dr. Kortlandt, a Dutch researcher, did a number of experiments with wild chimpanzees in natural populations in Africa. One of these was to see how different populations of chimps react to predators. To do this, he used a stuffed leopard dummy with electrically moveable head and tail. A baby chimpanzee doll

was placed in the leopard's front paws and the dummy was placed where it would be encountered by mixed groups of chimpanzees, including females with young, in all the experiments. Several populations of chimpanzees were so tested several times, including groups in two different jungle areas, and a group of savannah woodland chimpanzees. All the chimp groups reacted by picking up sticks as clubs, breaking small trees and tree limbs to use as clubs, and throwing these at the leopard dummy. An interesting difference emerged between the jungle chimps and the savannah chimps. The jungle chimps, while aggressive toward the leopard, were uncoordinated in their attacks and when throwing objects, never actually hit the leopard.

Dr. Kortlandt observed:

"The results with savannah chimpanzees, however, were quite different. They grabbed the largest of the available clubs, which was 2.10 m long, and they tore down small trees of about the same length; they slashed viciously at the leopard with these. With the aid of the film we made, we could measure impact velocities of approximately 90 km/h, which would have been sufficient to break the back of a live leopard. In addition, there was teamwork in evidence during these attacks, again in contrast to what we observed in the jungle chimpanzees. During the final attack the dummy was encircled by five chimpanzees, while two others stood in readiness at some distance, in case they should be needed. Then the leader grabbed the tail of the leopard and ran away, tossing the predator so that the head flew from the body.

"With that, the enemy was considered 'dead'. The apes showed no more fear of it, and the youngsters were allowed to touch it. The attacks on its head, however, continued during the whole day.

"A side effect of the experiment was the observation that the savannah chimpanzees more often walked erect than do the jungle chimpanzees."

Kortland, A.; pp. 44-46, in *Grzimek's Animal Life Encyclopedia* vol 11: Mammal II Dr. Bernhard Grzimek, ed. Van Nostrand Reinhold: New York, Cincinnati, Toronto, London, Melbourne.

Perhaps this difference is the result of being able to see your potential predators, an great advantage over having them sneak up on you unawares.

Chimpanzees do not seem to be overly afraid of big cats; besides the cases of chimpanzees occasionally sleeping on the ground while in leopard country, there are other reports that show their reaction. Dr. Kortland did another test in the sixties

with a stuffed leopard with a chimp doll in its paws; two female chimps threw sticks at it, apparently distressed at the plight of the "youngster". Bands of chimps have been spotted screaming at lions (from a distance) but holding their ground.

Then there was that leopard den. A band of chimps went over to a leopard den (sounds like "a gorilla walked into a bar..."; well, they really did; this took place at Mahale on October 3, 1984) and started screaming up a storm outside the den. This chimp group included not only adult males, but also females and young. From the sounds inside, the researchers observing this determined the mother leopard was inside at the time (but they didn't crawl in and check for some reason ;-). A couple of male chimps did, however, and dragged out one of the leopard's babies and beat it to death. During this period, some of the chimps were within 3 meters of the den's entrance, and this group included not only males, but several females, one with an infant.

So chimpanzees are not so afraid of large cats as we might reasonably suppose them to be. (That's understatement.) In fact, speaking about predation on chimps by large carnivores, Mariko Hiraiwa-Hasegawa et al. write:

"Most of the evidence is, however, only circumstantial and the predation pressure to the apes, if ever, seems to be extremely low. Moreover, chimpanzees have been observed to show 'offensive' rather than 'defensive' behavior toward such predators."

pg. 12, Hiraiwa-Hasegawa, Mariko; Richard W. Byrne, Hiroyuki Takasaki, and Jennifer M.E. Byrne, 1986, "Aggression toward Large Carnivores by Wild Chimpanzees of Mahale Mountains National Park, Tanzania", pp. 8-13, in *Folia primatologica*, volume 47.

Richard and Jennifer Byrne note:

"Researchers studying chimps have seen them encounter leopards a number of times. Sometimes, the chimps hardly react to the sight of a leopard, but usually they call aggressively and have managed to chase leopards away. In the daytime, leopards seem to be somewhat wary of the apes, even though, at up to 200 pounds, the cats are much larger than the primates, which weigh 70 to 85 pounds."

pg. 22, Byrne, Richard W.; and Jennifer M. Byrne, 1988, "Leopard Killers of Mahale", pp. 22-26, in *Natural History*, volume 3.

Aquatic Predators -- Crocodiles

The various AAT/H accounts make many statements about the lure of this water environment for safety from predators. My point is that rather than being a haven from predators, this environment would not only expose such hominids to a great number of predators, but also predators which seem much harder to defend oneself from.

Both crocodiles and sharks which are highly dangerous to humans even today are to be found in both fresh and salt water in Africa. And keep in mind that our transitional hominid ancestors were smaller than we are and also didn't have even such relatively "sophisticated" weapons as primitive hand-axes (ie., sharp-edged rocks).

The following quotes are from:

1989 *Crocodiles and Alligators* Various editors and contributors: Consulting Editor, Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) Facts on File: New York and Oxford.

My additional notes will be in bold face and brackets **[]**, like this:

[question: how do we know there were crocodiles in eastern Africa at the time of the transition?]

"Evolution and Biology: Evolution". Chapter by Dr. Eric Buffetaut (Director of Research, Centre National de la Recherche Scientifique, University of Paris, France)

pp. 37-38:

The ancestry of the Nile Crocodile (*Crocodylus niloticus*) and, to some extent, of the African Slender-snouted Crocodile (*Crocodylus cataphractus*) is known with some precision because of abundant crocodile fossils from the Tertiary and Pleistocene of northern and eastern Africa.

[Okay, they were around (and had been for some millions of years), but would crocodiles be in the sorts of habitats that the purported aquatic hominids were in?]

"Behavior and Environment: Habitats". Chapter by Dr. Angel C. Alcala (Professor of Biology and Director, Marine Laboratory, Silliman University, Philippines) and Maria Teresa S. Dy-Liacco (Research Assistant, Marine Laboratory, Silliman University, Philippines)

pg. 136:

Crocodylians are amphibious vertebrates, spending part of their lives in water and part on dry land. They inhabit rivers, lakes, ponds, marshlands, swamplands, and estuaries.

pg. 137:

The edges of freshwater lakes and ponds, where the water is shallow, receive abundant sunlight and therefore abound in rooted and floating plants that in turn support a diverse fauna. These edges of lakes and ponds, or marshes, are favorite haunts of crocodylians, since they rely on both water and land for their activities. The still waters of the lakes and ponds offer crocodylians a habitat that is rich in food organisms.

The lower reaches of rivers, where the water is slow moving, relatively warm, more saline, and well-stocked with plant life, also provide adequate cover and a good supply of food for larger aquatic organisms, including crocodiles. Freshwater and mangrove swamps are often well developed in these lower reaches.

[Well, that about covers it; they were in every type of habitat that these small hominids have been said to possibly have been in.]

This quote is from a sci.anthropology.paleo newsgroup post by Elaine Morgan:

"On the matter of crocodiles .. Instead of all these hypothetical crocodiles, read what really happens when the extant tribesmen in the Danakil area need to cross a crocodile-infested river today. They wade across it, with their cattle. The apparent reason is that the river is swarming with tilapia fish. Your average croc will not go through the strenuous business of grabbing the leg of a large mammal and threshing

and twisting around to tear it off, if he is full of tilapia and simply not hungry. (Source: Africa's Rift Valley, in the World's Wild Places series by Time/Life books)"

[my note: reading the above mentioned book one is immediately struck by Elaine Morgan's research style; it is in this case at least, I must say, somewhat lax. The passage doesn't mention cattle, but rather "camels and goats", and the fish are catfish, not tilapia.

But the question is not really whether or not Morgan gets her facts straight, or even whether author Collin Willock can actually spot "apparent fear or even watchfulness" from his perch in a helicopter flying overhead, but rather whether Nile Crocodiles are, as Morgan implies, merely peaceful fish eaters who wouldn't harm a small aquatic biped. Let's see what the experts say.]

"Evolution and Biology: Living Crocodilians". Chapter by Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) and Dr. William Ernest Magnusson (Research Scientist, Instituto Nacional de Pesquisas de Amazonia, Brazil)

pg. 67:

Nile Crocodile (*Crocodylus niloticus*)

Habitat: Known to occupy a wide variety of freshwater habitats, this species also frequents coastal areas in West Africa, and in southern Africa one was spotted nearly 11 kilometers (7 miles) off the Zululand coast. From time to time crocodiles are washed out from East African river mouths to the sea; some of these have been able to cross to the island of Zanzibar **[note: Zanzibar is 22 miles off the coast of Tanganyika, which together form the republic Tanzania]** and crocodiles are occasionally found on beaches or river mouths in Kenya.

Distribution: The widespread crocodilian of the African continent, it is found throughout tropical and southern Africa, and Madagascar. its historical distribution included the Nile River delta and the Mediterranean coast from Tunisia to Syria. Isolated populations of the Nile Crocodile are known to have existed in lakes and waterholes in the interior of Mauritania, southeastern Algeria, and northeastern Chad in the Sahara Desert.

Diet: Very large animals eat antelope, zebra, warthogs, large domestic animals, and humans.

[I'm not suggesting that the aquatic hominids were romping about in Asia, and note only that the Indopacific Crocodile may have had a wider range at one time. The main reason I've listed info on them is that it gives a clear picture of the abilities of crocodiles to live even in oceanic waters, travel great distances, and colonize new areas. It has also been found that in areas where Indopacific Crocodiles *do not* live, such as when and where they were hunted out in the 1950s and 1960s, other crocodiles took over those habitats, including salt-water habitats.]

pg. 68:

Indopacific Crocodile (*Crocodylus porosus*)

Habitat: Commonly encountered in marine habitats, the common names Estuarine or Saltwater Crocodile are, however, misleading since this species is often found in freshwater habitats such as large rivers and lakes.

Distribution: The most widely distributed of all living crocodilians, the Indopacific Crocodile is found throughout the tropical regions of Asia and the Pacific, wherever there is suitable habitat. Its distribution is still not fully known but recent research suggests that it is found from the islands of the Indian Ocean, coastal India and Sri Lanka, through mainland Southeast Asia, the Indonesian and Philippine islands, northern Australia, New Guinea, as far as the Belau Islands and perhaps Fiji in the Pacific Ocean. The ability of this species to survive in the open ocean has enabled it to reach, and sometimes colonize, many small islands such as the Cocos Islands (nearly 1,000 kilometers from land) and the New Hebrides. Stories of these crocodiles out in the open ocean abound and some individuals have been seen with pelagic barnacles attached to their scales.

Diet: Large animals eat whatever they want, including a variety of mammals and some birds. Fish also form part of the diet.

[More on food and hunting styles of these crocodiles.]

"Behavior and Environment: Food and Feeding Habits". Chapter by A.C. (Tony) Pooley (Consultant on Crocodile Farming, Conservation, and Education, Scottburgh, South Africa)

pg. 83:

The Nile Crocodile has an extremely wide range of mammals in its diet ranging from cane rats to Cape buffaloes, which can be as heavy as itself. Mugger, Indopacific, and Nile crocodiles also include humans in their food range.

pg. 83

Crocodilians are usually regarded as idle hunters that lie in wait in the offshore shallows for the approach of unsuspecting prey. They rely on camouflage and an ability to lie submerged with only their nostrils, eyes, and ears above the surface to scent, see, and hear approaching prey. From this concealed position they can launch themselves out of the water with astonishing speed, and may make a rush of several meters up a beach to snap at prey approaching the shoreline. Even large crocodilians are capable of vaulting almost vertically out of the water to a height of more than 1.5 meters (5 feet) to snap at birds or animals on river banks above them.

pg. 86:

Sometimes the crocodile erupts from the water into the midst of a herd of drinking antelope then uses its massive bony head to deliver sledge-hammer side-to-side blows to stun prey, break limbs, or knock an antelope into the water.

pg. 86:

Many of the larger crocodilians are known to take advantage of the habitual behavior of their prey, taking up position in the water on regularly used game trails, or at well-used river crossings or watering sites for humans and livestock, particularly if previous attacks at these sites have been successful.

pg. 86 (picture of crocodile attacking a wildebeest, captioned):

On their long migrations vast herds of wildebeest have to cross many streams and rivers. With so many animals to choose from this crocodile does not even bother to submerge before attacking.

pg. 90:

In other Zululand rivers similar behavior may be seen in summer when

rivers flood and water spills over into channels leading to natural pans. The crocodiles form a barrier where a channel enters the pan, facing the intruding water and snapping up river fishes such as bream (genus *Tilapia*) and catfish.

[Okay, it's clear that crocodiles, and Nile Crocodiles in particular, are not the peaceful, fish eating, Disneyesque creatures Morgan suggested. But surely they aren't really capable of killing and eating humans! Keeping in mind that the humans of today, even those with the most primitive of weapons, are better armed than our earliest hominids.]

"Crocodilians and Humans: Attacks on Humans". Chapter by A.C. (Tony) Pooley (Consultant on Crocodile Farming, Conservation, and Education, Scottburgh, South Africa), Tommy C. Hines (Consultant on Alligator and Crocodile Management, Florida, USA), and John Shield (Veterinarian, Cairns, Australia)

pp. 173-174:

Adult Nile Crocodiles weighing as much as 1,000 kilograms (2,200 pounds) and up to 6.5 meters (21 feet) in length have been recorded. The Nile Crocodile has evolved on a continent where it has had to contend and compete with a far greater variety of competitors, as well as potential prey species, than any other crocodilian in the world. Competitors in the aquatic habitat include predatory fish and sharks, monitor lizards, three other crocodile species, and hippopotamuses. On land, it needs to defend territory, nest sites, and offspring against a range of predators and competitors ranging from small mongooses to elephants, and, of course, humans. Consequently, the Nile Crocodile is one of the most aggressive of all crocodiles. In order to survive in habitats populated by such a diverse fauna, it has become a versatile and opportunistic hunter, and master predator of the aquatic environment.

[So it's good at its job, but what about defense? What about spotting it coming and getting out of the water?]

pg. 177:

Crocodiles, like sharks, are rarely seen prior to an attack. The attack is swift, silent, and without warning.

pp. 175-176:

Like most crocodiles, when lying motionless in the water, the Nile Crocodile is cryptically colored and difficult to detect. It regularly enhances this natural concealment by lying next to a stand of reeds, under an overhanging tree, among water lilies, or drifting alongside a floating object.

It can breathe, smell, see, and hear while only the top of its nostrils and the top of its head are visible above the surface. From this sit-and-wait position it will make the final lunge at unsuspecting humans or antelope that approach the water's edge.

Alternatively, the crocodile may detect a potential victim when it is drifting some distance offshore. It will submerge and approach closer and closer, swimming underwater and bringing its head to the surface perhaps once or twice to check the location of the prey. The final lunge may carry the attacking crocodile several times its own length up the beach. Acceleration imparted by the powerful tail is combined with a simultaneous forward swing of the hind legs as the crocodile beaches. The toes and feet dig into the bank and the powerful legs lever the body upward. If the bank is steep, the crocodile appears to vault straight out of the water. If the prey is still out of reach, the hind-leg stride may be repeated and the crocodile may lower its head and hook it over the top of the bank to support its body for another stride. Many an unsuspecting antelope or relaxed fisherman has been seized in this form of attack, even when 1.5 meters (5 feet) above the water.

[Okay, what about safety in numbers?]

pp. 176-177:

Contrary to popular belief, noise or safety in numbers does not deter a crocodile attack. Of the attacks investigated, only five of the victims were alone when seized. Several of the victims were snatched from amidst groups of men, women, and children who were either fording rivers, washing clothes or food, or bathing, and who were making a considerable amount of noise at the time. Field studies have shown that crocodiles are attracted from considerable distances to the sounds of a struggling animal in shallow water or to a shoal of leaping fish and that sound is often how prey is located.

Many witnesses and survivors of non-fatal attacks estimated that the crocodiles involved were in the range of 2.5 meters (8 feet) in length. These animals could be placed in the weight range of about 100 kilograms (220 pounds) and thus, weight for weight, the average human adult would stand a chance of escaping and surviving attack from a crocodile of this size. One could conjecture that these crocodiles were subadult or young adult animals. The injuries suffered by the survivors of these attacks also suggest that the crocodiles were small to medium-sized animals, for the wounds inflicted included the loss of hands, feet, and breasts, severe lacerations, and broken arms or legs.

A question from a newsgroup poster:

>>don't you think early man could have developed defensive
>>strategies to guard against crocodiles?"

[Okay, what about fighting it off?]

pp. 176-177 (continued):

However, the accounts of fatal attacks by large Nile Crocodiles -- 23 of the 43 attacks investigated -- indicate that these crocodiles were extremely aggressive and ferocious. There were several instances where crocodiles, having seized their victim, were either repeatedly stabbed with spears or knives, beaten with sticks, pelted with stones, or had sticks rammed down their gullets in order to prise the human victims from their jaws...but to no avail. In these attacks few bodies or remains were retrieved. Considering that a large Nile Crocodile may weight [sic] up to fourteen times that of an average human and can seize and drown Cape buffaloes as heavy as themselves, a human being, out of his or her element in the water, has little chance of surviving such an attack.

[The best defense seems to be not getting into trouble in the first place.]

[Now these are some accounts of the problem of crocodile predation on humans today. First note that this *doesn't* tell us anything about how likely it was to get attacked or killed millions of years ago, with numerous crocodiles and our ancestors being smaller and less well armed than we are now; what it *does* tell us is that, even with crocodile numbers being cut back due to habitat destruction and hunting, there is still a problem with these animals attacking and killing humans. (Estimates are that between

100-300 are killed each year in Africa by Nile Crocodiles and up to 1000 people a year throughout the range of the Indopacific Crocodile.)]

pp. 182-183:

Throughout the archipelagoes of Indonesia, the Philippines, and New Guinea, together with the adjacent land masses of Malaya and northern Australia, there is evidence that crocodiles have always exacted a terrible toll on villagers living close to the water. Only the problems of contact with isolated communities and poor communications have obscured the statistics on fatalities and when and how they occurred. Where reliable sources of information are available, they paint a picture of regular predation by crocodiles on villagers and even situations where entire communities have been terrorized.

A missionary at a village in northern Irian Jaya reported that no fewer than 62 of the villagers had been killed or maimed by a rogue crocodile in the 1960s. Six fatal attacks, and a great number of non-fatal attacks, occurred on Sarawak's Lupar River between 1975 and 1984. Tiny Siargo Island, off Mindanao in the Philippines, has reported the deaths of nine villagers in recent years, all possible victims of the same crocodile.

A fellow who lives part-time on Siargo emailed me and insisted I delete this account of the attacks on Siargo Island. Now I have nothing against deleting any info here that's wrong, but I didn't delete anything after his request. He just didn't provide any backup for his claim that these attacks never happened, and I found his claim unconvincing, especially compared to a renowned crocodile expert. First, he complained about Tony Pooley referring to Siargo as "tiny", and while I don't know as I'd describe it that way myself, I pointed out that it's far tinier than the island where I live. And if you lived on a "non-tiny" island how could you be so sure you'd heard all incidents of animal attacks? (I know I was surprised to learn how many cougar attacks had taken place on my island -- one of the world's hot spots for cougar attacks -- and even more surprised that a cougar had been found wandering about less than a mile from my apartment -- in the city -- only a few years before we moved here.) But the real problem is that he offered no evidence whatever, when it should've been fairly easy to do so, and hasn't -- despite requests -- for going on 6 years now. Please, if you want to have me change something you don't like, provide me with some evidence that you have a valid point.

As the Indopacific Crocodile grows, the mammalian component of its diet grows in proportion. An individual of 5 meters (16 feet) in length would

generally be quite accustomed to killing pigs and, occasionally, cattle, buffaloes, and horses. There are reliable accounts of leopards being killed. Except where it has been hunted, there is no reason to believe such an animal would recognize a human entering its territory as anything other than a potential meal.

pg. 183:

When a potential meal approaches, in the form of a bird or perhaps a dog, a hungry crocodile, apparently asleep on the bank, may slip quickly and silently into the water. By the time the "meal" arrives on the scene there is no sign of danger but the crocodile is watching quietly from under the water. If the intruder approaches closely enough, the crocodile explodes into action and grabs it in a ferocious rush that has been compared to the eruption of a Polaris missile from its underwater base. The crocodile seizes the prey animal with its jaws locked onto the head, muzzle, leg, or whatever part of the body is within reach. If the impact of this first contact is not sufficient to disable the prey animal, the crocodile tries to drag it into deeper water. The notorious "death roll" may be used at this stage to unbalance the unfortunate victim. Once the contest has moved into the water, the advantages are with the crocodile and the victim is drowned or killed by crushing bites.

pg. 186 (picture of a river landing, captioned):

Adult Indopacific Crocodiles are territorial and will defend their territory against intrusion, even by boats. A local woman, only ankle-deep in the water beside this landing stage on the Daintree River, Australia, was taken by a large male crocodile in December 1985.

[This was a typical attack; neither the victim or either of the two men standing beside her heard or saw anything until she was grabbed and flipped off her feet too quickly to even make a sound.]

Lastly, a newsgroup poster made this suggestion:

>>These eggs would make a very good meal for early hominids and it would
>>be an effective way to control the population of crocodiles as a whole.

[Now, since crocodiles undergo a lot of predation on eggs (and young as well), yet didn't decline in numbers until massive habitat destruction and hunting began a few hundred years ago, this doesn't make sense. Crocodiles have evolved to withstand massive losses of young -- about 90% or more.]

Behavior and Environment: Mortality and Predators Chapter by A.C. (Tony) Pooley (Consultant on Crocodile Farming, Conservation, and Education, Scottburgh, South Africa) and Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA)

pg. 92:

During the first six months, and up to several years of life, hatchling and juvenile mortality from predators is high. In fact several African and Pacific crocodile management programs justify harvest of eggs and/or juveniles as recruitment of this segment of the population into the adult breeding population is so low. Some wildlife management specialists estimate a death rate as high as 90 percent during the first year of life. However, as a crocodile successfully runs the gamut of early life predators and reaches adulthood, the number of predators and the causes of mortality diminish rapidly. An adult crocodile has little to fear but its peers and humans.

pg. 95:

In Africa, the Nile Crocodile (*Crocodylus niloticus*) nest predators have been described from various countries and habitats. These include water and white-tailed mongooses, honey badgers or ratels, olive and chamca baboons, otters, warthogs, bushpigs, and spotted hyenas. Avian predators of nests include the Marabou Stork, which has learned to probe through the sand with its stout bill to remove eggs from unattended crocodile nests. However, the Nile or water monitor lizard is undoubtedly the major predator of crocodile eggs throughout the African continent and, over some nesting seasons, these lizards may be responsible for stealing up to 50 percent or more of all eggs laid.

Aquatic Predators -- Sharks

Both crocodiles and sharks which are highly dangerous to humans even today are to be found in both fresh and salt water in Africa.

This section describes the habitats and habits of sharks.

I must say, after reading about both sharks and crocodiles, that I feel that predation by crocodiles would have been a greater problem for the AAT/H's water-based transitional hominids than predation by sharks. There are three main

reasons for this: first is that sharks, while they do often come into quite shallow water, are more common in greater depths of water. Second is that sharks seem to attack humans only partly for food; partly it's out of an apparent defense of what it considers its territory. Third is that attacks may only consist of one or two bites; but of course leaving a hominid crippled might well be essentially the same as killing it outright.

In contrast, crocodiles like to live in shoreside waters just where the AAT/H-proposed hominids are postulated to have lived; crocodiles generally stalk and kill mammals, including humans, as food; and crocodiles, once they get hold of their victim, do not let go, even when being pelted with rocks, stabbed with knives and spears, or when sticks are rammed down their throats. Crocodiles are serious about their job.

The information quoted here regarding shark attack comes from chapters about shark attack in many areas of the world, including the US, Australia, and South Africa. This information is applicable to all tropical and sub-tropical areas in Africa (the area in question here) as all three of the species of sharks which swim into and/or frequent shallow waters (and which are consequently most dangerous to humans) live in those tropical and sub-tropical African waters, as well as being widespread throughout the world.

Again, my additional notes will be in boldface and brackets **[]**. **[Like this.]**

The following quoted material is from:

1989 *Sharks in Question: The Smithsonian Answer Book* Springer, Victor G. (curator in the Department of Vertebrate Zoology, Division of Fishes, National Museum of Natural History, Washington, D.C.), and Joy P. Gold (Technical Information Specialist at the Department of Vertebrate Zoology, National Museum of Natural History, Washington, D.C.) Smithsonian Institution Press: Washington, D.C. and London.

[Some people say crocodiles don't live in salt water; we've already seen that is wrong. Some also say that sharks don't live in fresh water; that is wrong as well. In fact, one of the most dangerous of sharks (the Bull Shark, sometimes also called the Zambezi Shark) lives in both fresh and salt water.]

pp. 66-67:

Ichthyologists (scientists who study fishes) have been interested in the sharks that occur in freshwater. Formerly, sharks found in Lake Nicaragua, Lake Izabel in Guatemala, the Ogowe River in western Africa, the Zambezi River in eastern Africa, the Tigris River near Baghdad, and the Amazon River of South America, to name but a few, were each believed to be a different species that was confined to freshwater. Recent studies, however, demonstrate that in all these rivers and lakes there is but one species: the bull shark, *Carcharhinus leucas*, which is also commonly found in the warm, shallow waters of the oceans. In Lake Nicaragua, which drains into the Caribbean Sea by the broad Rio San Juan, the bull shark apparently moves freely between saltwater and freshwater. Even the rapids along the river seem to present no barrier to its passage. The bull shark has been implicated frequently in shark attacks on bathers, especially in warm freshwaters such as Lake Nicaragua and the Zambezi River, which have access to the sea.

[Is this shark dangerous to humans?]

pp. 116-117:

Bull Shark

Of all the sharks swimming the continental coasts of tropical and subtropical seas, the bull shark, *Carcharhinus leucas*, may be the most dangerous. In number of attacks on humans, it is one of the three species of sharks most often implicated. It may not have the white shark's reputation, but its large, heavy body, huge jaws, and very large teeth make it meter for meter just as formidable. Even though it appears to move slowly when cruising the shallows inshore, it is capable of fast, agile movements when it wants to attack prey. In Natal, South Africa, the bull shark is particularly notorious as an aggressive species, and it is caught four times more frequently than either the white or tiger sharks in the protective antishark nets set off the beaches.

pg. 117:

The species is relatively large, purportedly growing to a length of 3.4 m, but with an actual record of only 3.2 m (based on a report from Brazil). Individuals over 3 m are rare.

pg. 160 (from Appendix Three: Lengths of Selected Sharks):

Bull Shark (*Carcharhinus leucas*)

at maturity: male 1.57-2.26 meters; female 1.8-2.3 meters;
maximum total length: recorded 3.2 meters, reported 3.4 meters.
smallest free-living 0.56-0.81 meters

pg. 117:

The bull is the only shark known to live in the saltwaters of the oceans, the brackish water of estuaries and river mouths, and the far upper reaches of freshwater rivers and lakes. Even its newborn young and juveniles enter freshwater without difficulty. It travels from the Caribbean 100 km up the Rio San Juan to Lake Nicaragua, where it was long thought to represent a different land-locked, endemic species. It has also been reported from Lake Ysabel in Guatemala, the Mississippi and Atchafalya rivers in the southern United States, 3,700 km up the Amazon to Peru, the Gambia River on the middle upper west African coast, more than 550 km from the sea up the Zambezi River on the middle lower east African coast, the Tigris River of Iraq, the Hooghly River of northeastern India, and Lake Jamoer in New Guinea. These are but a few of the bodies of freshwater frequented by bull sharks. As recently as August 12, 1985, a large, 2.7 m bull shark weighing about 216 kg was caught in the Chesapeake Bay along the western North Atlantic seaboard.

[So they're fairly big, and they live virtually everywhere. What do they eat?]

pg. 118:

Bull sharks feed on a variety of prey and are almost as omnivorous as the tiger shark, the ocean's junk food eater; however, the bull is less likely to eat indigestible garbage. It favors bony fishes and other sharks, particularly young ones in the nursery grounds. In the sea, it will also kill and eat dolphins and sea turtles. In rivers, it has been known to kill hippopotamuses. Even the remains of large land animals, including antelope, cattle, dogs, and humans have been found among its stomach contents.

Dr. Leonard Compagno, who was the ichthyological consultant to the film *Jaws*, believes that the bull shark is quite possibly the most dangerous of all sharks, and that many attacks attributed to other species may actually have been committed by bull sharks. The famous, closely spaced series of five attacks -- four fatal -- that occurred in New Jersey in 1916,

have been attributed to the white shark. At least three of the attacks, however, occurred two miles up a narrow tidal creek, a habitat type in which no white shark has ever been confirmed to occur. A tidal creek is a likely place for bull sharks to be found, and few other species of sharks have been reported from such a habitat. No other western Atlantic shark species is known to enter tidal creeks.

[They're big, they live virtually everywhere, and they attack and kill humans even today. They're one of the main reasons South Africa and Australia have erected expensive anti-shark nets on their beaches.]

pg. 108:

How does the white shark attack its prey? Generally by surprise, and from behind and below, sometimes in an accelerated headlong rush at 17 kph to 25 kph, sometime inverting on its back or rolling to its side, according to the size and posture of its prey.

The following quoted material is from: 1987 *Sharks*. Various editors and contributors: Consulting Editor, Dr. John D. Stevens (Senior Research Scientist, Division of Fisheries, CSIRO Marine Laboratories, Hobart, Tasmania, Australia) Facts on File: New York and Oxford.

[The following covers much the same ground, and simply confirms that the three species of sharks in question, the bull shark, the tiger shark, and the great white shark, are all dangerous to any animal which habitually uses shallow waters of seas. This especially holds true for the bull shark, which is highly dangerous to any animal which uses the waters of not only seas, but also estuaries, and rivers and lakes with an outlet to the sea. In other words, the habitat of the aquatic ancestor postulated by the AAT/H.]

"Shark Attack in Australian Waters". Chapter by Roland Hughes BSc (former editor, *Australian Natural History*, Sydney, New South Wales, Australia)

pg. 110:

Coppleson concluded that the swimmer attacked by a shark is usually either a lone bather, part of a small group or on the edge of a large group. Shark attacks can occur in all kinds of weather, on dull days and

fine days, at high, medium or low tides, in clear, muddy or even brackish water.

pg. 116-117:

Bull or Zambezi Shark

Carcharhinus leucas

Because it is less physically impressive than either great white or tiger sharks, the bull shark's reputation as a danger to humans has been underemphasized. However, it is abundant in tropical and subtropical seas, estuaries and even freshwater rivers (specimens have been collected 3700 kilometers from the sea in the Peruvian Amazon). It frequents shallow water near beaches, and is a versatile and opportunistic feeder that will attack without provocation. The bull shark is a stout to heavy-bodied species that grows to 3.4 metres in length, and is now known to be the culprit in attacks formerly blamed on the elusive Ganges Shark (*Glyphis gangeticus*).

Tiger Shark

Galeocerdo cuvier

Regarded as the most dangerous shark in tropical waters, the tiger shares with the great white and bull sharks membership of the 'unholy trinity' of proven maneaters. It grows to about 6 metres and is responsible for repeated attacks on swimmers, divers, and boats.

Great White Shark

Carcharodon carcharias

Primarily a coastal and offshore species of continental and insular shelves, the great white grows to about 6 metres and is regarded as second only to the orca, or killer whale, as a marine predator. It has been identified in attacks on humans off California, southern Australia, New Zealand and South Africa.

"Shark Attack in the United States". Chapter by Dr. Guido Dingerkus (Director, Natural History Consultants, Goshen, New York, USA)

pg. 123:

One of the most dangerous sharks, and in tropical waters unquestionably the most dangerous, is the tiger shark (*Galeocerdo cuvier*).

pg. 123:

Off Bimini, in the Bahamas, the author has caught six of these sharks, ranging in length from four to six metres, in only one or two metres of

water. It is, therefore, a species that poses a serious threat to bathers in shallow, warm waters.

pg. 124:

The bull shark, *Carcharhinus leucas*, is probably the next most dangerous shark in warm waters.

pg. 124:

Coming into shallow water, it will feed on almost anything it encounters, swallowing smaller prey whole and using its razor-sharp teeth to bite circular chunks out of larger prey. It is probably the only species to enter pure fresh water, and has been caught as far as 3200 kilometers upstream in the Amazon River. It is especially aggressive in fresh water, where it apparently comes to feed. In Lake Nicaragua many human deaths have been caused by this shark.

All these factors probably mean that the bull shark comes into close association with humans quite frequently.

"Shark Attack in South Africa". Chapter by Dr. Leonard J.V. Campagno (Senior Research Scientist, J.L.B. Smith Institute of Ichthyology, Grahamstown, South Africa)

pg. 137:

At least some writers have thought the Zambezi shark the most dangerous species in the warm waters of Natal, and responsible for many attacks off bathing beaches there. In terms of relative abundance and habitat the Zambezi shark is, or was, more likely to come in contact with humans than the tiger or great white sharks, which also occur in the warm inshore waters of Natal. Although smaller than the other two, the Zambezi shark has very large teeth, massive jaw muscles for its size, eats almost as wide a range of food as the tiger shark, and occurs in a greater variety of inshore habitats than the other species.

"Shark Attack in New Zealand". Chapter by Larry J. Paul BSc (Hons.) (Fisheries Research Division, Ministry of Agriculture and Fisheries, Adelaide, South Australia)

pg. 152:

How does one avoid becoming a shark statistic? Only by never going into the water, for sharks are unpredictable and no measures give total security.

[Although sharks may not have been quite as much a danger as crocodiles for a relatively small, comparatively slow swimming and defenseless aquatic ancestor, the AAT/H needs to address the extremely serious threat to these habitually aquatic creatures posed by *both* sharks and crocodiles.]

Skin, sweat, and glands

Morgan and other AAT/H proponents have long maintained that the patterns of human skin glands, apocrine, eccrine, and sebaceous, can only be explained by an aquatic past. However, their claims that aquatic mammals lose their apocrine glands and sweat-cool via eccrine glands, and that human sebaceous glands are a waterproofing adaptation, are all contradicted by the facts.

Difference between the skin of seals and humans

The outer layer of seal skin is composed of "flattened, solid, keratinized cells" (King 1983:143, *Seals of the World*; British Museum (Natural History), Oxford University Press). "This may be contrasted with such a layer in man for example, where enzymatic digestion of the non-keratinous parts of the outer cells of the stratum corneum results in these cells becoming mere flakes continuously shed. Such a condition in a seal could lead to the skin becoming waterlogged" (King 1983:143, *ibid.*).

Clearly human skin is very unlike that of hairless aquatic mammals; this waterlogging of the skin in humans is why your fingers wrinkle in water. The specialized skin of these seals is what allows the sebaceous gland to provide its waterproofing function. That these glands are incredibly different in seals compared to humans and other primates can also be seen by examining their life history, which you'll see in the section on sebaceous glands below.

"Aquatic eccrine sweating"

This section looks at another of Morgan's oft-repeated claims, and examines whether it has any basis in reality.

From Elaine Morgan 1990:94, *The Scars of Evolution* Souvenir Press: London:

"There is even one clear aquatic instance of eccrine sweat-cooling. When the fur seal goes ashore to breed it suffers from the heat because of its

double insulation of fat and fur. It is believed to be descended from a possibly dog-like land-dwelling ancestor with foot-pads of the standard mammalian type: the flippers are covered with eccrine glands which sweat profusely as it waves them in the air to cool itself."

From Elaine Morgan post 10 Nov 95 (in sci.anthropology.paleo):

"James Borrett asked me for a reference to the statement I gave that harp seals use eccrine glands for thermoregulation. I was quoting from memory, and I'm sorry I got the wrong seal. It is the fur seals that do this. When they haul out and are overheated on land they sweat through eccrine glands on their naked flippers."

"The reference is: G. A. Bartholomew, in 'Vertebrate Life', McFarland et al, London: Collier MacMillan, 1979, p.773"

This is the ref I mentioned in my opening page; we find that Morgan has provided not so much an actual reference but instead a "find a reference" kit (assembly required). First you have to find the book she got the reference from, because that's all the info she gives. Once you find the book, you can look for the actual Bartholomew reference. When you find that, what you find is that you are *actually* looking for Bartholomew and Wilkie's 1956 article in *Journal of Mammalogy* 27:327-337. Now you can embark on looking up the actual reference.

Now when we look at the reference Morgan has given, we find an interesting thing: there is no mention whatsoever of eccrine sweat glands. This is not especially surprising if you realize that seals have *no eccrine sweat glands at all*. Why does Morgan assume, contrary to fact, that seals have eccrine sweat glands? The answer undoubtedly lies in her oft-stated belief that apocrine glands disappear in aquatic species (this was a long-held cornerstone of her version of the aquatic ape hypothesis). Seals, however, are well supplied with apocrine sweat glands (Scheffer 1962; Ling 1974; Sokolov 1982; King 1983). Note that reference "Sokolov 1982" is a book which Morgan uses extensively -- when it suits her purposes.

In fact, in the Bartholomew and Wilkie article, as in the book *Vertebrate Life*, the authors don't even actually notice sweat on these seal flippers (which Morgan claims "sweat profusely"). They actually mention that the seals wave the flippers about, and that the flippers are "abundantly supplied with sweat glands" (which we've already seen are apocrine rather than eccrine sweat glands). Is there actual evidence that these seals' flippers produce any sweat at all?

In a 1965 article in *Nature* (which Morgan cites in *The Scars of Evolution*), John Ling mentions the evidence for actual production of sweat due to heat in these seals: "the appearance of liquid droplets on the glabrous palmar surface of the northern fur seal, *Callorhinus ursinus* (L.), after exposure to radiant heat was assumed to indicate that sweating had been induced" (Ling 1965:561). His reference is the 1962 work of Victor Scheffer, in which Scheffer describes the experiment that led to this conclusion:

"When a heat lamp is focused on the naked flipper of a freshly killed seal, the black epidermis soon begins to blister. Before it does so, droplets appear on the surface of the skin in a fairly regular pattern. These are assumed to be secretions of the sweat glands." (Scheffer 1962:7)

So the evidence shows that if you fry a dead seal flipper until it blisters, the apocrine glands exude fluid. This is what Morgan claims is "one clear aquatic instance of eccrine sweat-cooling".

Discussion

I have here done what Morgan has repeatedly claimed she wants done: treating the AAT/H as befits a serious scientific theory. When doing a critique of any sort of theory, you check the facts the author uses to support the theory.

In this section we see another example which demonstrates that Morgan's claims are based on, at best, really poor research. This is not an isolated example; we can see this in her claims about sodium, emotional tears, and salt glands. In all these instances she has made claims that are contrary to the facts, and indeed has done so even when the facts are contained in the very source she inaccurately cites. She also persists in making phony claims even after facts (with references) are pointed out to her (for instance regarding salt glands being present in terrestrial birds and reptiles as well as in marine forms). This is either the mark of a dishonest researcher or an incredibly poor researcher. Yet it is this standard of research and evidence Morgan offers as a reason to throw out the last quarter-century of paleoanthropology.

For anyone familiar with Morgan's published and online writing, it is also evident from this particular reference that her standard of evidence for her own theory is far less stringent than what she expects from all others.

Morgan has often made the claim that apocrine glands are more efficient for cooling than eccrine glands, but ignores the advantage of eccrine glands. Apocrine glands

discharge their contents and these must be replenished before they're used again, while eccrine glands can go on and on. This allows humans a huge endurance advantage over apocrine sweating animals. The famous example of !Kung hunters running down a giraffe is an example of this advantage. The giraffe's apocrine glands work for only so long before they must recharge, and then the animal must rest or become overheated; this allows the far slower but steadier humans to outlast it and run it down. But does this mean that humans must always be near water? Note that both among the !Kung and Australian aborigines, hunters would often go on long treks far from water without carrying any water, and this in far drier country than our ancestors lived in. Even in the strawman version of savannah ("arid, treeless") that Morgan and other AAT/H proponents use, carrying water would be unnecessary.

Marc Verhaegen makes his own "seal sweat" claim

Marc Verhaegen has taken up the cudgel with a claim he continually repeats in online forums: that after humans, the "most-sweating mammals" are sea lions (he sometimes uses the phrase "most-themoregulatorily-sweating"). He ignores such well-known and well-established sweaters as horses, and the also well-established but perhaps less well-known (as sweating animals) primates such as gorillas, chimps, and baboons. He says sea lions are the thing, and he gives a reference for this claim, so we can check it out and see if it supports his claim.

One thing I will say for him: his reference is relatively clear by AAT/H advocates' standards and even refers to the page numbers. On the other hand, he often mistakenly claims that the mammal in question is the Northern Fur Seal (it isn't), and he often makes the same mistake Morgan does of saying these are eccrine glands (they aren't). He has sometimes used the same bad reference that Morgan used in the above example, and he apparently often confuses the two references. But he seems to have settled on a different reference once people started, apparently with the help of this site, pointing out the problems with the other one. This certainly adds confusion to the story, but nothing we can't manage; so it's time to hit the library.

First thing here is to give ourselves some extra time, because Verhaegen's now-preferred reference is "p.87-88 in AR Hoelzel ed.2002 'Marine mammal biology' Blackwell". The reason for the extra time is that this is obviously a generalized textbook (just as Morgan's reference was), not a journal or other volume where we'd be more likely to find the primary data one needs to make the claim that Verhaegen did. So we need to get the textbook, then find the papers that the textbook refers to. How do we know that there's going to be more of a search?

Call it intuition, or experience, or just look at the general statement that Marc quotes, the one he claims says that sea lions are second only to humans in sweating: *"sweat glands on the flippers of otariids aid in heat transfer. On hot days sealions & furseals can often be seen fanning their flippers & increasing evaporative heat loss at these sites"*. Now note what this statement *doesn't* say -- it doesn't say anything about how much sweat sea lions produce, whereas Marc obviously wants us to think it does. So we know that after we get the book, we're also going to have to find the further studies that the book refers to, the ones Marc didn't mention and probably didn't read. Note too, if you have a critical mind (and in science you'd better have such a mind) that it looks like Marc left something off the front of that first sentence. I don't know about you, but I want to know if he did, and if so, what it says. So we're off to the library.

Okay, got the book, and here's the pages; turns out this is Chapter 3 of a general textbook on marine mammals, this chapter by Terrie Williams and Graham Worthy from UC Santa Cruz and U of Central Florida, respectively. And Marc got the quote right, but there *are* two words missing from the front of that first sentence. The words are "In addition". That's interesting, because they're obviously talking about sweating not being the be all and end all of thermoregulation you'd expect it to be in the second "most-thermoregulatorily-sweating" animal in the world. The part before the sentence is the bulk of the paragraph and talks about the really neat blood vessels many pinnipeds have in their skin, especially around their flippers. These are called AVAs (arteriovenous anastomoses) and what they do is open and close off blood flow to either conserve or disperse heat -- blood gets hot as it travels through the body and the blood vessels cool off when they're near the surface rather than when they're deep in the body core, so if a seal needs to conserve heat, it restricts these AVAs, and when it needs to get rid of heat, it opens them up. It works exactly like a radiator, and is a critical part of thermoregulation in seals. In addition, they sweat... but how much? Must be a lot to be the second "most sweating", since we know that other mammals, such as horses, etc., manage to sweat away their heat in very hot conditions. So does this textbook say something about it, some numbers maybe? Why yes, it does, and look at that, it says it just after the section that Marc quoted. I wonder why he didn't quote that sentence?

Oops, I see why. It seems they don't sweat very much at all: *"Evaporative cooling resulting from both sweating and respiratory losses accounts for less than 20% of heat production in California sea lions studied under experimental conditions (Masuura & Whittow 1974; South et al. 1976)."* That's not very sweaty. It turns out that sea lions really don't sweat very efficiently or effectively at all. We can go to the next step now and get some more facts and figures, from the references the

textbook cited, the ones Verhaegen probably didn't bother to read (we know he didn't tell us about them).

This is exactly the sort of thing one needs to look at, especially when the text contradicts what you're claiming, as this textbook contradicted Marc Verhaegen's claim. The two studies referenced are slightly different in their goals; the South et al. study was aimed at finding out much of the heat that sea lions did lose was due to evaporative cooling (not just sweating but panting, and other techniques I'll mention later) and how much due to simple radiation and conduction-convection (this latter with the help of those nifty AVAs). The Matsuura and Whittow study was aimed at finding out how much of the heat the animals needed to lose actually could be lost through evaporative cooling.

I'll start with the South et al. article: Sea lions, like other otariid seals (eared seals) just can't get rid of the heat they produce and absorb even through panting and sweating *combined* -- under fairly typical conditions of 15-20 degrees C (about 60-70 deg F) those two methods *combined* can only lose about 20% of what they do lose and in fact in really hot conditions they do a sort of ersatz "sweating" by drooling all over themselves and urinating on themselves (hey, it's not easy for other animals to stay cool) and even with that to help out they can't do the job with evaporative cooling (it can nudge them up toward 50% of the total they lose but only under pretty hot conditions -- 28 degrees C, or about 80 deg F). Even with all that -- panting, sweating, *and* drooling and urinating on themselves -- the test animals just can't lose enough heat (more on that when we get to the next article) and so had a "precipitous rise in body temperature" which resulted in stopping the experiment. The seals were lying prone and panting heavily -- they didn't want to kill them. By contrast, we see that non-human primates, and horses (and certainly humans), among others, certainly manage much hotter conditions than that without taking to the water, which is ultimately how sea lions and other seals ordinarily handle a high heat load -- they hit the water before they hit the temperatures used in that test.

The other article referred to in the textbook also measured sea lion evaporative cooling, in this case to see how much was due to panting and how much to sweating and how much of the total they needed to lose could actually be lost through this combined method. They measured the sea lions at temperatures between 13 and 30 deg C (about 55-85 deg F) and found that the heat loss due to evaporation (both panting and sweating) was a bit under 20% of the heat produced by metabolism, which they note was "relatively ineffective" (the harbor seal they tested did even worse). The South et al. article emphasized that this amount was at least *something*, even if it wasn't nearly enough. The Matsuura and Whittow study showed that about 16% was due to sweating and about 2.5% due to panting. The

conclusion has several statements that are directly apropos to Marc's claim, which makes it a pity he didn't look at either of these primary sources which would've showed him his error. Let me quote:

"The total evaporative water loss from the sea lions in a warm environment could account for the dissipation of less than 20 per cent of the heat that the animals were producing. In contrast, many terrestrial mammals and birds are able to lose heat equivalent to their entire heat production by evaporation of moisture (Dawson & Hudson 1970; Hart 1971; Dawson 1973). If the minor sweating responses of pinnipeds are the legacy of their carnivory ancestry (see above), then the ineffectiveness of evaporative cooling mechanisms largely represents the absence of panting or of saliva spreading, in pinnipeds as opposed to terrestrial carnivores." (pp.18-19)

and:

"The absence of effective evaporative cooling mechanisms in sea lions has been discussed elsewhere (Whittow et al. 1972; Whittow 1973). Teologically, sea lions may attempt to conserve water rather than to maintain a constant body temperature. In the course of their adaptation to the sea, dehydration may have had a role in suppressing the evaporative cooling mechanisms that ancestral pinnipeds may have possessed." (pg. 19)

Discussion

Marc Verhaegen made the claim that sea lions sweat more than any other mammal except humans, which, since we know that other mammals sweat enough to stay cool in sometimes extremely hot conditions, must mean they sweat effectively enough to get rid of the body heat they produce. The textbook he used as a basis for this claim not only doesn't back him up, it directly contradicts him. And the further references that the textbook used give further proof, through actual experiments and measurements, that his claim is false. In fact, the authors of those papers refer to the sweating of sea lions as "minor" and "ineffective" and mentions how "the absence of effective evaporative cooling mechanisms in sea lions" was apparently related to "their adaptation to the sea" as the sweat cooling adaptations that their (terrestrial) ancestors may have possessed was "suppressed". And they contrast this unfortunate situation in sea lions to the "many terrestrial mammals and birds [which] are able to lose heat equivalent to their entire heat production by evaporation of moisture".

As with Morgan's "eccrine sweat" claim examined above, we see another example in which demonstrates an AAT/H proponent's oft-repeated claim turning out to be based on, at best, really poor research. Worse, the information that contradicted

the claim was contained in the same chapter that Verhaegen quoted from, one very short sentence after the sentences he quoted. This suggests that Verhaegen must have seen the clearly stated contradictory statement, but simply didn't report it. This in turn suggests that the fact he didn't do so was not an accident. And that, readers, is not good.

Even if you fully intend to do a good job and report the whole thing, unlike Marc's expurgated version of his reference, you have to watch out when you use a general textbook like this one. Mind you, I read more of it and it's a good book by good people, but even good people can make mistakes, and Williams and Worthy made one with the reference they gave for the first sentence that Marc quoted (he left the references out of his quote). The statement that Williams and Worthy make about seals flapping their flippers is "supported" by what can only be an error, since the article they refer to (Blix et al. 1979) makes no such claim and is in fact *only* about how newborn harp seals manage to keep from freezing since they're born on ice floes in the wintertime (short story on that: they shiver a lot). But I know from actual sources (Bartholomew and Wilkie 1956, for instance) that seals do in fact wave their flippers when they get hot (this isn't a controversial statement, and doesn't actually need the degree of support a controversial or new statement, like Marc's sweating claim, needs). But this does point out the danger of using secondary references, especially for information that's either not widely known or well established. Authors sometimes make mistakes, as Williams and Worthy obviously did here.

But how do we know that Verhaegen didn't also simply make a mistake with his claim? If he had, it might be embarrassing that he has made his incorrect statement many times using references that don't support it, but it would still be simply a mistake, no matter how substantial or embarrassing. But in fact we know that it wasn't a mistake, because the information that contradicted his claim was not only in the same book, and not only in the same chapter, and not only on the same page, but actually in the paragraph immediately after the sentences he quoted. He couldn't have missed it, and that means it definitely wasn't a mistake that he didn't mention it, it was deliberate. So we can see he claimed the reference said something when it actually said something completely different, and he did this deliberately.

Sebaceous Glands and the AAT/H

AAT/H proponents claim that human sebaceous glands are an aquatic adaptation; that they waterproof the skin as in phocid (largely hairless) seals. They say that the

distribution of sebaceous glands in humans is just where you'd want them to be in an aquatic hominid. There are several reasons this doesn't make sense.

There's the fact that many aquatic mammals -- whales and manatees, for instance -- have either no sebaceous glands or only a very few, very rudimentary, ones.

There's the differences in seal skin (where they can provide waterproofing) and humans (where they can't) as noted above.

Finally, the features of sebaceous glands in humans suggests they are a sexually selected feature, as is common in mammals. There is a large difference between male and female humans in the number of their sebaceous glands. Males have a lot more of them -- as they do apocrine glands, which are also used as a sexual "lure" -- and complimenting this, females have better scent receptors.

This view of sebaceous glands also makes sensible another otherwise insensible fact -- their life history, ie. the age at which sebaceous glands initially become active:

In mammals which also utilize sebaceous glands in waterproofing skin or fur, these glands are numerous and become active by the time this feature is needed. In seals, for instance, they are active at birth. In humans, on the other hand, sebaceous glands are incredibly scarce until puberty and they only become active at puberty.

These facts leave us with two alternatives:

1. The "aquatic" hominids didn't use their environment until puberty, and females didn't use their environment nearly as much as males; *or*
2. The sebaceous glands of humans are a sexually selected trait as in other primates and many other mammals.

The second alternative is the only sensible one, and it's bolstered not only by the features of sebaceous glands, but also a lot of research on pheromones in relation to sex.

In this, humans are like other primates and many other terrestrial mammals, not like aquatic mammals.

How does Elaine Morgan respond when facts like these are pointed out, facts from a source she used as evidence even though it didn't say what she claimed it did? It's instructive to see; I mentioned the above facts about seal sweat in the

sci.anthropology.paleo newsgroup, and this part of Morgan's reply shows how she either misunderstands or misrepresents information from an accurate source.

Elaine Morgan post (sci.anthropology.paleo: 10 Dec 1995):

EM> I am glad you have been reading Ling. Did you enjoy the bit on page

EM> 562 about how "the need for waterproofing the skin is met by enlarged

EM> lipid-secreting sebaceous glands"? Did it remind you at all of the

EM> way humans have very numerous greatly enlarged lipid-secreting

EM> sebaceous glands which none of the other apes possess? Isn't that

EM> interesting?

EM> Elaine

Adapted from my reply to that post:

Morgan was making an incorrect assumption in claiming that these seal sebaceous glands are as large as those in humans. Ling, of course, as the book Morgan was reading from clearly states, was comparing the size of these glands in the various types of seals, and he was pointing out that they are larger in phocids than in other seals; he was not making a comparison with any other mammals. But as Montagna and Parakkal, who did make such comparisons, point out, "Of the many mammals studied, only lemurs have [sebaceous] glands about as numerous and large as man's" (Montagna and Parakkal 1974:285, *The Structure and Function of Skin*, Academic Press).

There are other problems with the hypothesis, which Morgan has made many times, that the sebaceous glands in human skin have waterproofing functions as seen in seals. One is that this hypothesis would require that infants and children forego this waterproofing -- a disastrous adaptation, to say the least; women also tend to have much smaller sebaceous glands. Further, the structure of seal skin is quite different than human skin; human skin doesn't have the hard outer layer that allows seals to be waterproofed by their sebaceous glands (see King quote above). Another tip if you want to do useful science: A hypothesis which is contradicted by facts which are well known at the time the hypothesis is constructed is a hypothesis which shouldn't see the light of day.

Conclusion

AAT/H proponents seem surprised to find that humans have features, such as hymens and large sebaceous glands, which resemble our primate relatives, the lemurs. Although for the past 40 years or so, scientists have realized that humans have a mosaic of "primitive" and "advanced" traits, AAT/H proponents insist that these traits can only be explained by convergence with the likes of seals and whales, which not only have been aquatic for 25 million years or more, but which don't really resemble us in these traits. As well, the AAT/H's peculiar position on the matter of eccrine sweat glands is that the progression we see amongst primates in numbers of eccrine glands (fewer in monkeys, more in apes, more in humans) is not due to relatedness, as an evolutionary theory would suggest in such a case, but rather is due to convergent evolution with distantly related aquatic mammals, and that the fact that most of these aquatic mammals seem to have very few if any eccrine glands is simply another of those things that need not be explained.

References

- 1956 "Body Temperature in Northern Fur Seals" by Bartholomew and Wilkie. *Journal of Mammalogy* 37:327-337.
- 1962 *Pelage and Surface Topography of the Northern Fur Seal*, by Victor B. Scheffer. North American Fauna, Number 64, U.S. Fish and Wildlife Service.
- 1965 "Functional significance of sweat glands and sebaceous glands in seals", by John K. Ling (Antarctic Division, Department of External Affairs, Melbourne, Australia). *Nature* November 6, 1965, 208(5010):560-562.
- 1974 "The Integument of Marine Mammals", by John K. Ling. In *Function Anatomy of Marine Mammals*, Volume 2, edited by R.J. Harrison. Academic Press: London, New York, San Francisco.
- 1974 *The Structure and Function of Skin*, Montagna and Parakkal, Academic Press
- 1979 *Vertebrate Life* (3rd edition 1989), by Pough, Heiser, and McFarland Macmillan Publishing Company: New York and Collier MacMillan Publishers: London.
- 1982 *Mammal Skin*, by V.E. Sokolov. University of California Press: Berkeley.

1983 *Seals of the World* (2nd edition), by Judith E. King. British Museum of Natural History, Oxford University Press.

1985 *Social odours in mammals*, Volumes 1 and 2, edited by Richard E. Brown and David W. MacDonald, Clarendon Press: Oxford.

1974 "Evaporative heat loss in the California sea lion and harbor seal", D.T. Matsuura and G.C. Whittow, *Comparative Biochemistry and Physiology*, vol. 48A, pp. 9-20

1976 "Air temperature and direct partitioned calorimetry of the California sea lion (*Zalophus californianus*)", Frank E. South, R.H. Luecke, M.L. Zatzman and M.D. Shanklin, *Comparative Biochemistry and Physiology*, vol. 54A, pp. 27-30

1979 "Some aspects of temperature regulation in newborn harp seal pups", Arnoldus Schytte Blix, Hans J. Grav, and Keith Ronald, *American Journal of Physiology*, vol. 236B (Jan-Jun) pp. R188-R197 (not apropos to this discussion as it's about baby seals staying warm rather than anything about cooling but included here so you can check me out if you wish)

2002 "Anatomy and Physiology: the Challenge of Aquatic Living", Terrie M. Williams and Graham A.J. Worthy, (relevant pages, pp.87-88, Chapter 3 in *Marine Mammal Biology*, A. Rus Hoelzel, ed., Blackwell Publishing

Can AAT/H proponents' research be trusted?

The typical person curious about science who comes across an article, internet post, or web site propounding the AAT/H sees some claims about physical features of humans and other animals about which they have little or no personal knowledge, and quotes from authorities whose work they haven't read. They generally assume these claims and quotes are accurate, and because of this they find the argument for the AAT/H convincing. They should be able to make that assumption safely, but unfortunately, they can't. The AAT/H is a hotbed of false "facts" and misleading, often altered, quotes to back them up. Other sections on this site have shown AAT/H proponents making false claims using phony "facts", often said to be drawn from good sources which actually correctly present real, and contrary, info.

This page is in two major sections: The first section looks at just a few quotes by leading AAT/H proponent Elaine Morgan (there are, sadly, many more where these came from) as examples of the most common techniques of changing quotes, and how they affect the argument. The second section shows how Morgan took one excellent source of information and represented it as saying the opposite of what it actually said.

Section 1: Elaine Morgan's misleading quoting techniques

With 4 books -- one a best-seller -- and many articles and talks on the subject so far, Elaine Morgan is the best known and most widely read AAT/H proponent. Her research and writing is behind most of the pro-AAT/H questions and statements you see in classrooms or on the internet. Because of this, it is important to examine the quality and accuracy of her work. Many times, AAT/H proponents such as Morgan cite the research of, and quote from, appropriate experts, leaders in their fields with good research behind their work. But these researchers find their work distorted by AAT/H proponents til it's said to say something it doesn't. An example of part of this process can be seen when we look at the quoting techniques of the AAT/H's leading proponent. Here we look at her use of quotes from scientists and ask whether she accurately reports what she reads.

This isn't easy because she very rarely gives page numbers for quotes (sometimes she doesn't even give the source of the quote at all), so there's a lot of searching to do. But I have been able, with diligent searching, to examine some of the quotes Morgan has used, and have found that she commonly distorts them in several ways, including leaving words out of them without indicating that words are missing. This is considered a cardinal sin when quoting, and Morgan, as a Oxford University literature graduate (I hear it's not a bad school) and professional writer since 1955, realizes this.

She has claimed that:

"I have never even in my green and unregenerate days omitted words from even the most long-winded quotation without indicating by a row of dots that words had been removed."

(E. Morgan post, 27 May 96 in the sci.anthropology.paleo newsgroup)

I knew this statement was untrue because I had checked for just such unindicated omissions in several of her quotes of scientists, both in her published work and her online posts. However, I hadn't realized the extent of her omissions in the quotes I had examined so far until she made that claim. When I saw her claim that she had never left out words from quotes without indicating it, I went back and checked 4 that I had at hand and which I knew had some missing, unindicated words. One short quote was missing just 1 out of 12 words; not too bad, except that it's a key word, which makes it seem rather convenient to have been the one to have gone missing. The other 3 quotes, however, were missing some pretty big chunks: in these an average of 29% of the words were missing -- not indicated in any way. Dropping 27 words from the middle of an 86 word quotation of Darwin and

pretending it's whole is just not kosher, and certainly contradicts Morgan's self-righteous claim.

Morgan also occasionally changes words in quotes -- another no-no. For instance when she quotes Darwin on page 60 of *The Scars of Evolution*, besides leaving out those 27 words, she also changed a couple of words in a manner that just happens to give a more adaptionist spin to his quote.

There are two possibilities here, neither particularly complimentary to her. She is either doing it on purpose and is therefore dishonest, or she does it by accident and is therefore incredibly sloppy. (Which is it? I don't know; I only know that her altered quotes just happen to support her arguments, while the unaltered ones don't.) Either way it makes her research and writing unreliable, and if her research and writing is unreliable -- if her facts are unsound and her quotations inaccurate -- how likely is it that her conclusions are good?

There are a couple of examples of Morgan's quoting technique which suggest that she isn't just being sloppy. One is that Morgan occasionally manages to precisely excise those words which would damage her case, as she did in an online quote of Sir Victor Negus.

Or she might simply use a non sequitur quote, in which the quote she gives as support for a claim is about another subject entirely. This is the case, for instance, when she quotes Elsner and Gooden as support in her passage regarding breath-holding when diving. The quote she gives as evidence isn't about breath-holding at all; it speaks only of heart-rate slowing (bradycardia).

She also uses a technique I had never noticed before, creating a *false context* for a quote. She does this most notably in the Darwin quote I mentioned (it's a little long to show here at present; I may put it online later), which she proceeds with several paragraphs which imply that the quote is Darwin's view from 1859, when he wrote *On the Origin of Species*. But Darwin actually wrote those words in his 1871 book *The Descent of Man, and Selection in Relation to Sex*. That Morgan intends to convey this false impression can be seen when she refers to this quote in her next chapter; there too she implies that it was said before Darwin's 1871 book ("he hadn't changed his mind that..." Morgan 1990: 71). Another example of Morgan creating false context is her quote of Derek Denton seen below.

Perhaps the most troubling part of her quoting of Darwin is that the quote she gives is a introductory statement in which Darwin uses a devil's advocate approach to set up his detailed explanation (this method is a common stylistic element in Darwin's writings). Morgan quotes only this introductory statement, and in doing this she duplicates a very common, very dishonest, ploy often used by creationists, most

often seen when they quote the first part of Darwin's famous passage about the evolution of the eye. It is disturbing to see these creationist-like tactics being used in Morgan's work.

Keep in mind that these misleading quotes are just a few examples among many.

In the following quotes the excised portions and/or relevant portions which were excluded are in **boldface**.

Morgan quotes Elsner and Gooden: Accurately?

From 1990 *The Scars of Evolution*, by Elaine Morgan. Souvenir Press: London.

pg. 139 (Chapter title is "Breathing"):

"Aquatic mammals, however, have necessarily acquired more conscious control over the operation of their lungs. Unlike land mammals, they can voluntarily regulate the timing of the breaths they take and the amount of air they inhale. Land mammals' breathing reflexes respond only to events which have already happened, as in the case of an animal which accidentally falls into the water. A diving mammal like a seal or a dolphin purposefully regulates its breathing in relation to actions it intends to perform, as explained in a report by R. Elsner and B. Gooden on diving asphyxia:

"Some major component of the diving response is determined by the intention, conditioning or psychological perspective of the animal being studied. Thus, the diver acts as though it produces the most intense diving response when the need for achieving maximum diving duration is anticipated."

Morgan uses this quote as support for her claims about conscious control of breathing. However, when the quote is in context, it is clear that Elsner and Gooden are talking only about bradycardia -- slowing of the heart rate -- not breath control.

1983 *Diving and Asphyxia: A comparative study of animals and man* by Robert Elsner (Professor of Marine Science, University of Alaska) and Brett Gooden (Adelaide, South Australia. Formerly Lecturer in Physiology, University of Nottingham). Cambridge University Press: Cambridge.

pg. 4:

"There was usually less decline in heart rate during trained immersion than during forced immersion (sea lion and harbour seal), but in dolphins the bradycardia was intensified during trained dives. The generalization which appears to apply is that some major component of the diving response is determined by the intention, conditioning or psychological perspective of the animal being studied. Thus, the diver acts as though it produces the most intense diving response when the need for achieving maximum diving duration is anticipated."

Morgan quotes Sir Victor Negus: Accurately?

One of the experts Morgan uses quotes from is Sir Victor Negus. From an Elaine Morgan Post in sci.anthropology.paleo, Monday, Jul 31, 1995:

"Comparing ' for instance a fast-running horse or deer to a considerably slower Man, he says "in the latter there is a tortuous airway with sharp angles at the anterior and posterior choanae and at the larynx. In addition to tortuosity there is obstruction at the internal ostium and at the glottis. There are eddies produced in Man, esp. in the gap between the posterior nasal passage and the laryngeal aperture.."

Comparing Morgan's quote of Negus with the full, unaltered quote below, note that Morgan has deliberately excised all the references to the human fetus and various primates, as these references harm her thesis that the human airway's shape is due to convergent evolution rather than phylogeny. Note too that these deletions were not noted in her quote, which was presented as if it were unaltered. No wonder she gave no hint of what book, much less page number, her highly altered quote came from (it wasn't easy to ferret out).

From: 1965 *The Biology of Respiration* by Sir Victor Negus E&S Livingstone Ltd.: Edinburgh and London. pp. 123-124:

"Nevertheless, certain modifications are found which correspond with the powers of exertion of various species, for instance a fast-running horse or deer to the considerably slower Man. In the latter there is a tortuous airway with sharp angles at the anterior and posterior choanae and at the larynx as well seen in a human foetus; in addition to tortuosity there is obstruction at the internal ostium and at the glottis. **Sagittal sections of a Monkey show an intermediate**

condition, with an intranarial epiglottis to connect the posterior nasal passage and the larynx.

"There are eddies produced in Man, esp. in the gap between the posterior nasal passage and the laryngeal aperture. **A sagittal section of the head of a baboon (*Cynocephalus*) shows much the same points, but in lesser degree; in an Orang (*Simia satyrus*) the gap is greater.**"

In that same message, Morgan continued to misrepresent Negus's statements; she quotes:

"While the nose of many vertebrates has reached a high degree of perfection for humidification, for olfaction, or for rapid transfer of air currents, the organ in Man is a rather poor affair"

Reading only what Morgan quotes, one might be lead to believe that Negus suggests that humans are greatly different in this than their close relatives, which supports her usual claims. Actually, in comparisons throughout his book (ref below), Negus repeatedly uses variations of the phrase "in the higher apes and Man" when remarking on their similarities as opposed to the differences seen in other animals in so many of the features he describes. He often describes these features as he does in the paragraph (quoted below) which immediately precedes what Morgan quoted, in which he points out that when viewing a series of primates, from "primitive" to "advanced", one can observe this regression of features in the nose. This time Morgan didn't cut anything from the middle of the quote, she just started it late enough to remove the explanatory context.

From 1958 *The Comparative Anatomy and Physiology of the Nose and Paranasal Sinuses*, by Sir Victor Negus. E&S Livingstone Ltd.: Edinburgh and London, pg. 349:

"One can observe the signs of regression in the nose by inspecting a series of the Primates, starting with the most elaborate nose in the Tree Shrew; although described as the first order of the vertebrate kingdom, yet the description is not applicable to most of the timid representatives of the Primates, which took to trees to escape their enemies or to find food denied to them on the ground.

"And so we find, as this book should demonstrate, that while the nose of many vertebrates has reached a high degree of perfection for humidification, for olfaction, or for rapid transfer of air currents, the organ in Man is a rather poor affair..."

As an odd last note on Morgan's quotes of Negus that day, she ended with a passage she felt showed evidence for her contention that humans evolved in an

aquatic environment. I present it as another example of how she has so often made mistakes which manage to reach the exact opposite conclusions that the evidence she reads actually show.

E. Morgan: "While looking this up, I cam across this nice little comment: In aquatic species the odorous molecules are perceived when in solution in water. In terrestrial species a smell conveyed in pure aqueous solution is not perceived. But if dissolved in saline solution, the odour may cause stimulation.

"It is somewhat surprising that in terrestrial Man, salt is essential. Whether this phenomenon applies to terrestrial vertebrates other than Man does not appear to be determined."

The actual unaltered quote:

Negus, from pg. 92 of his 1958 book mentioned above:

"It is somewhat surprising that **while a fish living in fresh water is able to perceive odours carried in a non-saline medium, yet** in terrestrial Man salt is essential; whether this observation applies to terrestrial vertebrates other than Man does not appear to be determined.

Backman thought that to be odourous a substance must be soluble in water and in lipoids, so as to pass through the watery layer of mucus and then to be dissolved in the lipid material which constitutes the olfactory cells."

Now aside from this being yet another example of how Morgan alters quotes, leaving out important words and phrases without indicating it's been done, there are two interesting points in this one. One is that Morgan didn't see fit to include Negus's suggestion, citing Backman, that the saline mixture would allow the odorous substance to be soluble in the saline environment of the terrestrial animal's nose. The other is in the section she stated above, which is her rephrasing of Negus's preceding paragraph. Read it again, and consider these points (actual Negus quotes, Ibid, pg. 92):

"In aquatic species the odourous molecules are perceived when in solution in water, but Seydell states that in terrestrial species a smell conveyed to the olfactory area in pure aqueous solution is not perceived."

"If dissolved in saline solution, however, the odour may cause stimulation, according to Jerome."

"while a fish living in fresh water is able to perceive odours carried in a non-saline medium, yet in terrestrial Man salt is essential"

So while Morgan obviously thought Negus's statements were evidence that humans had an aquatic background, Negus actually said the *exact* opposite: that in aquatic species an odor in pure water can be perceived, but terrestrial species need the odor to be carried in salt water, and that humans need the odor to be carried in salt water. This rather obviously groups humans with terrestrial species rather than aquatic, yet Morgan managed to see it as the opposite.

Morgan quotes Denton: Accurately?

In the section on "Salt and the AAT", we saw how Elaine Morgan claimed Derek Denton's research on salt said the exact opposite of what it actually did say. Let's look at her quoting technique with Denton. When she quotes Denton in *The Scars of Evolution*, her distortion of his position is more subtle than her other claims (detailed in the sections before this one). For one thing, although she alters the quote without indicating her omission, she does only leave out one word; not 27-31% of the quote as she has in her quotes of other authors. Instead, she supplies a *false context* for the quote, making it seem to say something which Denton did not say.

Morgan quotes Denton, pg. 101:

"On the other hand, when an animal has had enough salt it will take no more. In humans neither the compulsory search nor the abrupt cut-off point can be relied on. Their intake bears no relation to salt deficit or surplus. This is surely not a characteristic that would have been acquired on the savannah. As Denton points out, in savannah conditions '...selective pressures would favour retention and perhaps elaboration of salt mechanisms'."

That's the context within which Morgan quotes Denton. Now I've already shown (in the section on "Salt and the AAT") that the claims Morgan makes in her first three sentences are false, and in fact are contradicted by Denton's work. But in this section we are concerning ourselves only with Morgan's Denton quote, so let's take this quote apart:

"As Denton points out, in savannah conditions '...selective pressures would favour retention and perhaps elaboration of salt mechanisms'."

From Morgan's lead-in phrase, "in savannah conditions", you might reasonably get the idea that Derek Denton had actually said something about savannahs pertaining to the quoted sentence, but this context which Morgan provides is in fact a complete fabrication. Denton neither says it nor implies it.

He refers instead to the "environmental, dietetic and metabolic conditions" which led to "the behaviour towards salt of a wide variety of species of wild herbivorous mammals", and he points out that these conditions could naturally have "operated on hominoids over a large part of their evolution". So he is not, as Morgan implies, referring to a relatively transient period of a few million years or to only "savannah conditions", but instead to a wide variety of environments and conditions over tens of millions of years.

That this very long time period, and the largely vegetarian (ie., non-salty) diet of our ancestors, is what he means is clear from the sentence that opens this section of Denton's book:

"The principal consideration in this discussion of the 30 million years of evolution of the Hominidae, is that diet was probably almost exclusively vegetarian during the first 25 million years."

(Denton 1982:70)

He also points out that given data from humans who gather and hunt today, "diet may also have been predominantly vegetarian over the last five million years, though there was considerable variation in diet according to the prevailing climate and conditions".

This lead-in fabrication ("in savannah conditions") giving a false context to Denton's words is just the first bit of tweaking Morgan does to make the quote say what she wants it to, and she hasn't even gotten to the quoted material yet!

Denton, *The Hunger for Salt*, pg. 70:

"It follows that selection pressure would favour retention and, perhaps, elaboration of salt **appetite** mechanisms..."

Now about the word "appetite" being missing in Morgan's book. Well, she also changed "selection" to "selective" -- maybe she's just a poor library researcher and proofreader. It is suspicious, though, that it just happens to be that the word "appetite" might well make many readers realize that these "salt mechanisms" are part of their own familiar, personal behavioral repertoire, rather than a "lost instinct" as Morgan wants them to believe. It also leads into the rest of Denton's sentence, which Morgan seems not have wanted her readers to see at all:

Denton, *The Hunger for Salt*, pg. 70:

"It follows that selection pressure would favour retention and, perhaps, elaboration of salt appetite mechanisms -- both hedonic liking and the hunger with deficiency -- which were developed at earlier stages of phylogeny."

The hedonic liking of salt, a very recognizable characteristic in humans. The hunger with deficiency as well is present in humans just as in other mammals, contrary to Morgan's claim, as Denton's book clearly demonstrates. Morgan's edited quote puts one in mind of what Philip Kitcher said about the quoting techniques of a pair of creationists:

"Whitcomb and Morris do not quote that sentence. Perhaps this is because it conflicts with the point they are trying to defend. Or perhaps we should accept the explanation that Gish is reported to have offered at the Arkansas trial: 'After all, you have to stop quoting somewhere.'" (Kitcher 1982:182)

It sure *looks* like Morgan was trying to hide something, but who knows; maybe, like Gish, she'd just say: "After all, you have to stop quoting somewhere."

Here's Denton's full paragraph; Denton, pg. 70:

"The behaviour towards salt of a wide variety of species of wild herbivorous mammals has been recorded in Chapter 3. The environmental, dietetic and metabolic conditions which determined it could have operated on hominoids over a large part of their evolution. It follows that selection pressure would favour retention and, perhaps, elaboration of salt appetite mechanisms -- both hedonic liking and the hunger with deficiency -- which were developed at earlier stages of phylogeny. This may also have held for the elaboration of salt retention by aldosterone secretion, which has a multifactorial mode of control that includes special adaptations to the assumption of the upright posture. This selection pressure would have been ameliorated by meat-eating with the consequential obligatory sodium supply during the latter 2-3 million years of hominid evolution. Whether this resulted in regression of these mechanisms to any extent is a matter of speculation (see Chapter 26)."

References

1982 *Abusing Science: The Case Against Creationism*, by Philip Kitcher. The MIT Press: Cambridge, Mass. and London, England.

1990 *The Scars of Evolution*, by Elaine Morgan. Souvenir Press: London.

1982 *The Hunger for Salt*, by Derek Denton. Springer-Verlag: Berlin, Heidelberg, New York.

Section 2: Elaine Morgan's misleading presentation of the major points from Derek Denton's research

This section covers some of the same subject covered in more depth on my "Salt and the AAT/H" page, but with an emphasis on the trustability of Morgan's published work. Here I juxtapose a few examples of Morgan's presentation of some excellent research by Derek Denton which he wrote up in a very good, and very easy to read, book, 1982's *The Hunger for Salt*. Reading Denton's clearly written statements followed by Morgan's claims made with Denton as the source, it's easy to see that she does not present what he said accurately (to say the least).

Morgan reads this:

Denton: "But in the human case, the cultural influences are acting in the context of an innate propensity: the liking for the taste of salt and readiness to ingest it."

and produces this:

Morgan: "A salt crisis in our evolutionary history would go far to explain one other specifically human characteristic -- the fact that we have no instinctive awareness of the state of the sodium balance in our bodies."

Morgan reads this:

Denton: "A characteristic feature of salt appetite is the delay in its appearance despite large rapid loss of sodium. With sheep the delay is usually 24 h, with rats 4-8 h. This is in striking contrast to thirst."

and produces this:

Morgan: "When we are suffering from a deficiency of water we feel thirsty, and take active measures to find a source of water and set the balance right. If the thirst

remains unslaked it intensifies until all other considerations become subordinate to the need to drink.

"Most mammals respond just as urgently to the need for salt if they are deprived of it."

Morgan reads this:

Denton: "In rats with sodium deficit as a result of adrenal insufficiency, the characteristic behaviour was to overdrink [a saline solution] considerably relative to deficit."

and produces this:

Morgan: "Derek Denton, in his classic study *The Hunger for Salt*, describes his researches into salt appetite in non-human mammals such as sheep, rats and rabbits. In all these species there is a precise correlation between the amount of salt their bodies need and the amount they will take in."

and this:

Morgan: "On the other hand, when an animal has had enough salt it will take no more."

And Denton made clear that salt appetite was something one sees in animal which are not carnivorous (because they get plenty of salt from meat and blood and don't need to seek it out) and not living at seaside (because they get plenty of salt from their food and don't need to seek it out). Salt appetite comes from evolving in a salt-deplete environment. Now Morgan, although she cited and quoted Denton and claimed he said the opposite of what he actually said about salt appetite in humans, did largely drop the claims after I'd pointed it out to her over a period of some weeks in the mid-1990s, but then we find her claiming in 2008 that "the immersion" could have been in saltwater, which means she's not actually dropping it, just having it as one option when it's been shot down, it's dead, it's an ex-parrot (no matter how beautiful the plumage).

The more fundamental problem revealed here, however, is that quite clearly Morgan took an excellent source of solid information and claimed it said the exact opposite. It's hard to see how this could have been done inadvertently, but it really doesn't matter whether it was inadvertent or deliberate. Inadvertent would mean a bad

scholar due to not being able to understand clearly described research. Deliberate would mean a bad scholar due to being dishonest. Either one means a bad scholar, one whose work cannot be trusted.

Wegener, Continental Drift, and unaccepted theories

What does Continental Drift have to do with the Aquatic Ape theory?

A history of Wegener's theory, suitably edited for effect, is becoming a standard for people with an unaccepted theory. (It's often coupled with the ever-popular "inaccurately rendered Piltdown story".) Morgan has put forward the Wegener comparison in *The Scars of Evolution*, and in many of her sci.anthropology.paleo newsgroup posts. Marc Verhaegen continues this tradition -- he actually started making the Wegener comparison with his introductory newsgroup post within days of first getting online, and since then he has done it at least 32 times (as well as putting forward occasional comparisons with Galileo, Copernicus, and Einstein). David Attenborough's April 2005 BBC Radio4 show on the AAT/H featured Wegener comparisons prominently.

According to the typical way of putting it, Wegener put forward a simple theory which said the continents had once been one, had split apart long ago, and slowly moved to their present locations. For this he was shunned and summarily dismissed by all science, only to be vindicated several decades after his death by scientists who realized that Wegener's theory was exactly what had actually happened.

Good story... too bad it didn't happen that way.

It seems that comparing themselves to Wegener is a popular pursuit with many people. One problem with their comparison is that the commonly expressed sentiment -- "no one supported Wegener" -- is wrong. It is also a fact that Wegener's proposed mechanisms were obviously inadequate, as evidenced by the fact that he himself admitted this was so in the 4th edition of his book. This part about the mechanism has special significance for supporters of the AAT/H who look to Wegener's theory as a icon, and I'll get to that in a moment.

Contrary to these comparisons, many people who disagreed with Wegener about his idea took it quite seriously: they disagreed in scientific journals and conferences. Wegener's idea wasn't widely accepted because there was no reasonable candidate for a mechanism capable of producing the effect. This is a perfectly reasonable

objection to a theory. When finally such a candidate was found, Wegener's underlying idea -- but not his inadequate mechanism -- was accepted.

Although some reaction was probably based on being hide-bound, a major reason for Wegener's idea not being accepted was that he had no reasonable mechanism, apparently no way for it to actually happen. Until a reasonable hypothesis regarding a mechanism was brought forward, it is reasonable to reject a theory. In Wegener's case, this took quite a few years, decades, actually, and this helped opposition harden against the idea.

Wegener was correct only in the initial part of his claim -- the part about *what happened*. He was dead wrong, almost laughably so, about the *mechanism*. His mechanism, a combination of pole-fleeing force and the tidal attraction of the sun and moon, was far far less powerful than would be necessary (several 1000's of times so), and Wegener admitted this himself in the 4th edition of his book.

There are two particularly interesting aspects of Morgan's and Verhaegen's use of Wegener's theory of continental drift to try to make her own position seem to be that of a misunderstood genius. One is that it contains a common logical flaw, one that I call the "Fulton's Folly Flaw", which arises when the "misunderstood" person cries: "They laughed at Fulton, and they laughed at me; therefore I am a great inventor!"

The second is that Morgan could hardly have picked a worse theory to compare hers with. The AAT/H claims that hominids evolved from an hominoid ancestor through the mechanism of an extended period of aquatic living. Paleoanthropology has also always suggested that hominids evolved from an hominoid ancestor, but rejects the mechanism of an aquatic past (well, except for the long ago aquatic past all life on earth shares). In other words, the theoretical difference isn't in *what happened* (hominids evolving from an hominoid ancestor), but in the *mechanism* for it. The *mechanism* (an aquatic past) is the different special thing the AAT/H brings to the table.

The problem here for Morgan and other AAT/H proponents using the Wegener comparison is that Wegener was correct only in the initial part of his claim -- the part about *what happened*. He was dead wrong, almost laughably so, about *the mechanism*.

In David Attenborough's April 2005 BBC Radio4 show on the AAT/H his Wegener comparison included a bit of business which seemed calculated to confuse; it was either severely ignorant (hard to believe of Attenborough) or outright dishonest

(also hard to believe of Attenborough). He referred to "Wegener's Plate Tectonics" as having been accepted (in suggesting the AAT/H might also be), but Plate Tectonics was not Wegener's idea at all. Wegener's idea was Continental Drift and lacked any reasonable mechanism; Plate Tectonics is the missing mechanism and was discovered long after Wegener had died. This is important for the reasons stated in the next paragraph, and it's odd (at least) that Attenborough conflated the two as they are two very different things.

So the history of Wegener's exposition of the theory of continental drift is anything but a palliative for supporters of the AAT/H. The problem with the AAT/H is not the *what happened* part; both the AAT/H and the rest of paleoanthropology agree that hominids evolved from a hominoid ancestor. The AAT/H is not offering a new idea there. The new idea the AAT/H offers is in *the mechanism* for this evolution, and it's in the mechanism that the AAT/H, like just Wegener's theory, falls flat.

Although it's common to claim that Wegener was universally derided and ridiculed, the facts are, as usual with facts, more complex. He certainly had support, especially in folks from the southern hemisphere where the fossil and geological evidence was most obvious, and these supporters were not nutty heretic types. Not everyone agrees; here's Elaine Morgan posting in sci.anthropology.paleo (Sept. 11, 1995):

El> Wegener asked a good question - why all the continents looked as if they
El> fitted together, not just in general outline, but matching in geological
El> detail and parallelism of wildlife. He came up with a good answer: he
El> proposed that the continents had once been one big continent which had
El> split up. Nobody worth mentioning would listen to him.

Let's have a look at just who Elaine Morgan considers to be "nobody worth mentioning":

List and comments from *Continental Drift: The Evolution of a Concept* by Ursula B. Marvin, Ph.D., Smithsonian Institution Press: Washington, D.C. (1973):

Prominent Wegener supporters included:

Arthur Holmes of Durham and Edinburgh universities, "he was among the earliest and greatest pioneers in developing the radiometric methods for determining the ages of rocks and minerals and the age of the earth itself"; Emile Argand, founder of the Geological Institute of Neuchatel, Switzerland; S. William Carey, professor of geology at the University of Tasmania; C.S. Wright; Lester King, professor of geology at the

University of Natal; Professor Reginald A. Daly of Harvard University, who Marvin calls a scientist "of unchallenged prestige"; Alexander Du Toit, Johannesburg, South Africa, author of *Our Wandering Continents*; Armadeus W. Grabau, "an American paleontologist and author of several textbooks on stratigraphy and index fossils"; Leonce Joleaud, French geologist; R.D. Oldham, "discoverer of the seismic evidence for the earth's core"; and Dr. W.A.J.M. Waterschoot van der Gracht, "a Dutch geologist and vice president of the Marland Oil Company".

All "nobodies" according to Elaine Morgan.

Why don't anthropologists mention the AAT/H much?

One of the questions people often ask is why the anthropologists who deal with human evolution don't deal with the AAT/H in their writings. First of all, we have to note that they do sometimes (more about that below), but also that there are a couple of very good reasons why they don't spend more time on it. One can be seen by looking at the quality of the evidence used to support the AAT/H. The web site so far corrects only *some* of the bogus info and false "facts" used as support for the AAT/H. If the proponents can't come up with better information than false "facts" like "rhinos are predominately aquatic" or "only marine reptiles and birds have salt glands", "only humans and aquatic mammals have hymens", etc. etc., what hope *should* they have of having their theory taken seriously?

The style of argument used for the AAT/H typically boils down to: "I'm saying this without good evidence; accept it or prove me wrong". This leaves the critic with the task of proving a negative, and of assembling all the evidence, including the evidence that the proponents should've gotten together in the first place. This is an unattractive prospect. (I hate to tell you how many hours I had to spend digging out the real facts about the AAT/H.)

This problem can perhaps be most easily understood by looking at the history of Fred Hoyle's claims about *Archaeopteryx*.

Hoyle and Archaeopteryx

Fred Hoyle was (he died in August of 2001) an eminent British astronomer who took it upon himself to claim that the Archaeopteryx fossils which showed feather imprints along with skeletal details were either frauds or mistaken identity. These claims were widely publicized due to Hoyle's stature as a scientist. The fact that he's an astronomer with no training, study, or other experience in paleontology,

ornithology, or indeed anything remotely connected to the subject didn't deter him from making this claim, nor did it deter the media from uncritically reporting it ("he's a 'scientist', right?").

To refute this claim, researchers were forced to spend several months going over old research, and repeating already-done research, in order to publish a scientific refutation of Hoyle's claims. This added no new information to science, it simply wasted time and money better spent elsewhere. Someone had to use up months of their research life just to refute an exceedingly poorly thought-out and completely bogus claim. (There's a link to a more detailed account of this affair on my links page.)

This example brings out the basic reason anthropologists don't spend time refuting the AAT/H, and Elaine Morgan in particular. The theory isn't worth their time and (generally meager) research money. Morgan's work entails writing up a strawman version of human evolution, then kicking it down while demanding that people defend that or accept her version. Meanwhile, her version of human evolution is chock full of false "facts".

The AAT/H theorists have also gone in for a classic technique among marginal theories, the shifting target. Specifically, the "aquatic apes" have become less and less aquatic over the years, from being fully acclimated to sea life, diving, etc., to seashore-dwelling waders, to denizens of the shores of streams and inland lakes. This change wipes out some of their arguments (which unfortunately weren't good to begin with) but even so these arguments continue to be made. For instance, the (false) claim that human sweating and tears are a marine adaptation to excrete salt doesn't work at all in a freshwater environment, yet even after moving their ape evolution scenario to fresh water, they often keep the salt argument. Another (false) claim is that bipedal locomotion could not have been successful without the support of water, yet even after the scenario moves to an ape wading along the shallows of the shoreline, the argument stays. The same goes for hair loss and hair alignment, fat, and other features.

There are other problems with the AAT/H that experienced researchers see immediately. The first is the remarkable lack of fossils for the time period involved. This is to be expected if the early hominids and proto-hominids were living in wet, forested areas, as these are not conducive to fossilization. If they lived on shorelines, however, as the AAT/H requires, the situation is far different. Shorelines are great places to get fossilized. Present-day shorelines are not often great places to find fossils, as water action breaks them up, grinds them down, and erodes them smooth. But present-day shorelines are generally not the shorelines of the past.

Getting fossilized usually starts with getting covered, and you can hardly beat a shoreline for this. If our ancestors were indeed commonly wading along the shores, we would expect to find them fossilized right and left. We don't.

Another immediately apparent problem with this wading, diving, etc. business is that of predators. In open forest or savannah, upright hominids would have little problem spotting predators. Predators in the shallow shoreline waters, on the other hand, are even today a big problem. The biggest culprits are sharks (mostly in salt water but also in rivers) and crocodiles (which can be found in both fresh and salt water). Rather than being the protective medium that AAT/H proponents imagine, shallow water is, for humans, one of the most dangerous places in the natural world. The AAT/H prefers to sidestep this issue or pretend that it doesn't exist.

Why don't professionals spend their time refuting the AAT/H?

There are a few other points I want to make about the problems of scientists dealing with refuting poorly thought-out theories, which also relate to why anthropologists don't spend a lot of time critiquing poorly drawn theories supported by bogus information, such as the AAT/H.

Even among scientists, as we've seen with Hoyle, there are times when assertions are put forth which are poorly drawn, yet, because they strike a chord, often of wishful thinking, they catch on and are repeated. Yet refuting them can be an exercise in futility. A good scientific review and critique is a lot of work, and takes a lot of time. But it's far easier to pop off with a theory that's poorly researched than it is to accurately go over all the things that the original theorist should've, and to provide a point by point refutation.

Then, generally, you can count on several things happening.

First, the original writer can write several articles or papers in the time it takes the reviewer to write one good refutation, because the original writer isn't doing the work they should (which would've shown them the holes in their presentation if they'd done so). So that writer has many more publications to show for their time, and their work tends to be viewed as "original", while the refuter's work, arguably more important, is "review/critique" stuff, and hence "unoriginal". This publication imbalance affects their careers, and although it shouldn't be that way, the original writer gets more credit -- after all, they did "original" work, and more of it, too.

Second, by the time the refutation is done, the original writer often has several new articles out. The refuter can never catch up; in fact, they must inevitably fall further

and further behind, as well as running the risk of seeming to have no career or original thoughts of their own, being perpetually on the coattails of the original writer.

Then, assuming that the refuter does eventually deal with all the original writer's various articles, that writer can simply say, "but that's old news; we're way beyond that."

The original writer can also, at any time, easily shift the essence of their claim (as in "less and less aquatic"), then claim that the refuters are misrepresenting the original writer's work.

This makes chasing such claims very unattractive, especially when the original writer is simply presenting their stuff in popular formats, without the rigor of pre-publication professional critique (as is done with articles via peer review, and books via sending them out to the pros and getting feedback).

Did scientists in fact examine the AAT/H?

Now that I've explained why anthropologists might not want to spend valuable and scarce time refuting the AAT/H, it needs pointing out that AAT/H of Hardy and Morgan actually *was* examined, which you can tell because it got plenty of cites, as many or more than most any work done in human evolution in popular venues -- books and popular science magazines rather than first rank journals. The idea that it wasn't examined is promulgated to mask the fact that anthropologists gave it attention; it just didn't get the acceptance it's proponents think is its due. This idea that it wasn't examined has become a myth -- one of several -- surrounding the AAT/H.

This mythology that has been built up around the AAT/H has been initiated by the idea's proponents, but often it's accepted and repeated by others without really looking at them to see if they're true. For instance there's the claim that Alister Hardy held back on the idea for 30 years because he feared making any controversial statement, usually said to be because he wanted to become a Fellow of the Royal Society. He's explicitly said this, on camera, and it's been repeated by many people afterward. But does it hold water? He said he came up with the idea in 1930, and we can grant the possibility that Hardy didn't want to do any controversial work before he got his FRS. He got that in 1940. What was his excuse for the next 20 years? He wrote "Telepathy and Evolutionary Theory" for the *Journal of the Society for Psychical Research* in 1950. That isn't controversial? In the 1950s he wrote other things about the paranormal and his belief that it influenced biology

and our evolution; that wasn't controversial? Really? He even got a knighthood in 1957. The notion that fear of controversy made Hardy hold back his aquatic theory when he was writing on far more controversial ideas doesn't add up -- it just doesn't fit the facts.

So too the claim that the AAT/H was ignored when what actually happened was that it wasn't *accepted*. To see that it wasn't ignored we can look at citations of Hardy and Morgan and compare them to others' work. Not to work by the superstars -- people like Goodall, Leakey, and Geertz, to name one each from primatology, physical anthro, and cultural anthro; they have an enormous number of cites (over 38,000 for Geertz for instance) -- but to other people with interesting ideas, whether well supported or not. We can even get an example from someone who presented most of her cited work in a more popular book form (anthropologist Helen Fisher's books). These show that both Hardy and Morgan got plenty of attention in academic quarters, and that's just the attention that went as far as citing them. And of course many others read them but didn't cite them, as happens with much, probably most, work.

Using Publish or Perish we can look at citation records; when we do we get these results:

First, Alister Hardy's 1960 AAT paper:
103 cites

Compare this to Alister Hardy's acclaimed marine biology work:
16 papers, 496 cites

And Alister Hardy's academic paranormal work:
20 papers, 452 cites

Right away you see that he has more cites per piece, far more in fact, for his AAT article as he did for his other work. In fact that one article amounted to 10% of citations of his work, even though that other work brought him an FRS and a knighthood.

Now Elaine Morgan's books:
5 books, 457 cites

How does this compare to a professional anthropologist writing mostly in relatively popular books? to answer we look at:

Helen Fisher
5 books and papers, 487 cites

Very close. Now how about a typical researcher who's done consistently excellent work (IMO) for over 30 years:

Wenda Trevathan
83 papers, 793 cites

Obviously both Hardy and Morgan got plenty of attention, and even plenty of citations of their work. They just didn't find their work *accepted*, which is really where the claim in the AAT/H mythology is flawed. They didn't get their ideas *accepted*, but that's pretty normal for ideas really and doesn't rouse people, so they morph the claim into it not being *examined*. But the citations show that this is not true. Like the Hardy "origin story" that fear of making a controversial statement about evolution kept him from bringing up the AAT for 30 years, it doesn't fit with the facts, if you bother to look at the facts.

There's another aspect of this mostly regarding the newer researchers, including but not restricted to Verhaegen and Kuliukas. They also complain they aren't getting what they feel is their proper due when it comes to attention. But where do they publish? Verhaegen has done a lot in *Medical Hypotheses*, a pay to publish journal which "takes a deliberately different approach to peer review", that difference being that *they don't do peer review* (it's a "different approach", I'll give them that). But the new "premier" journal for this study of human evolution has become *Nutrition and Health*, and by choosing this venue the authors who do so virtually guarantee their work will be unexamined. To get an idea of how far out on the margins this journal is, we can look at how many citations the journal as a whole gets, how many for the papers on human evolution in that journal, and compare that to some more mainstream journals.

First, all citations of *Nutrition and Health* from its inception in 1986 to September of 2009:

173 papers, 376 cites
24 years, 7.21 cites/yr.

The same time period for only the papers on human evolution:

Nutrition and Health -- evolution (1986-2009)
10 papers, 32 cites
24 years, 1.33 cites/yr.

Compare this to the same time period for a well known, but not top tier anthropology journal:

Folia Primatologica (1986-2009)
445 papers, 2657 cites
24 years, 110.71 cites/yr.

And then to two top tier anthropology journals, this time *for just the past 2 years*:

Journal of Human Evolution (2008-2009)

268 papers, 380 cites

2 years, 190 cites/yr.

Current Anthropology (2008-2009)

205 papers, 106 cites

2 years, 53 cites/yr.

For those journals I had to use just the last two years (actually slightly less) because if you use the same time period that N&H has been out the you get far more than 1,000 results (number of papers cited) and Publish or Perish can't retrieve more than that. Clearly *Folia Primatologica*, *Journal of Human Evolution*, *Current Anthropology* have a far greater impact than the journal AAT/H proponents have decided to use. They're also far easier to find. For instance WorldCat shows *Folia Primatologica* available in 318 libraries worldwide versus 65 for *Nutrition and Health*. (381 have *Journal of Human Evolution* and 1158 have *Current Anthropology*.)

Did Morgan at least break new ground regarding the role of females during our evolution?

Last is an oft-stated claim that, whatever the flaws in her theorizing, Morgan at least was the first to emphasize the importance of women and children, and the roles of mothers and offspring, in human evolution. If this claim were true, it might be at least a point for her, despite the fact that her theory is all wet, but this claim is in fact also not true.

Morgan didn't break new ground in her treatment of women and children (and in fact her treatment seems rather a lot like Desmond Morris's own poorly drawn *The Naked Ape*, which she has stated was her source for her interest in the "aquatic ape" idea). That ground had already broken during the 60s by researchers such as Thelma Rowell and Jane Lancaster (and also in a great deal of the excellent work on Japanese macaques by many, mostly Japanese, researchers), and perhaps most directly by Sally Linton in a paper ("Woman the Gatherer") which drew on the work of Richard Lee among the !Kung. Linton's work provided much of the inspiration for the later influential work of Nancy Tanner and Adrienne Zihlman. It's a shame that Morgan's supporters ignore the incredibly important and influential work of these pioneering women.

A brief critique of Morgan's 1997 book, *The Aquatic Ape Hypothesis*

Elaine Morgan's 1997 aquatic ape book -- her sixth -- is supposedly her best researched (according to her) so I've read it and here jotted down some notes of things that I could immediately see were errors, problems, or logical fallacies. This is instructive, since it shows just how badly done her work is, and why it isn't accepted as reasonable; if it's so easy to see major problems with something just at first glance, what hope is there that delving into it at length will turn up a useful scientific theory?

In this book, unlike her others, Morgan has often (though not always) tied her specific statements to references. You'll also notice she changed the subject from the Aquatic Ape "theory" to the Aquatic Ape "hypothesis". This first happened during the olden (mid-1990s) sci.anthropology.paleo days, when Elaine showed up and people tried to point out to her the many areas where her work fell short of, well, accuracy. When folks in the newsgroup also pointed out that her ideas were more properly called a hypothesis, she took it as a massive insult and an *ad hominem* attack (she called it a "new anti-AAT ploy"), and railed against the people who pointed out the difference between "theory" and "hypothesis". It seems she figured, like most lay people, that "hypothesis" means something like "crummy theory". It doesn't (for the differences, [click here](#)).

In chapter one Morgan sets right to work on a favorite technique of hers, constructing a savannah strawman. This is one of the classic logical fallacies (see the link to Steven's Guide to the Logical Fallacies on the links page). Rather than use the actual descriptions of environments anthropologists say was the transitional environment, and rather than use the actual definition of savannah, she carefully builds a false version of these because a false, "straw" version is easy to knock down. Although savannahs by definition are woodlands, and can be either dry or wet, Morgan's "savannah" is always an arid, treeless plain.

A short definition of savannah from the BioTech Life Science dictionary is "a type of woodland characterized by a very open spacing between its trees and by intervening areas of grassland"; see the savannah definition on my page "What is the Aquatic Ape Theory?" for more info. Suffice it to say that the definition Morgan insists be used is more than 100 years out of date.

One of the ways Morgan constructs her strawman is seen on pg. 16 where, referring to the fossil "Lucy", she says: "She did not die in a savannah habitat, but in a wooded and well watered area", contrasting "savannah" and "wooded and well watered", which is an incorrect contrast. This isn't a new technique for her; it's a staple in her work, for example, in her 1995 book, *The Descent of the Child*, she refers on pg. 158 to the "scorching heat of the open plains, the meagre vegetation, and the scarcity of water." Many other examples can be found in her books, articles, and online posts.

Morgan, by the way, has been corrected on this subject many times over the course of the past decade. Her response, honed during online interaction, can be seen on pages 17-18, where she misleads the reader by claiming that the definition of savannah used for the past 100 plus years is a new invention with which "younger anthropologists" have apparently been duped by their elders "throughout the ten years or so they have been studying the subject". She's done this before too, with phrases like "this new scenario". Again, on this subject she has been corrected many times. Her continuing with this wrong view, therefore, seems dishonest, although one can't be entirely sure it isn't just incredibly bad research on her part. But either way -- whether dishonest or incredibly poor research -- it doesn't make sense to insist that people accept an implausible theory based on it. It's also rather mean-spirited to take advantage of lay people's unfamiliarity with the subject by feeding them "false facts".

In chapter 2 Morgan continues a familiar complaint about the fossil Lucy's presentation; that Don Johanson and his coworkers made a point of leaving out the fact that the fossil site where Lucy was found also contained remains of crocodile eggs, turtle eggs, and crabs. She implies that this was a malicious sort of misrepresentation of Lucy's environment perpetrated by people out to hide evidence. That this evidence was "hidden" by publishing it in the official announcements of the fossil's discovery makes it seem they did a mighty poor job of it. Morgan, on the other hand, does rather better on her end, repeatedly talking up the eggs and the crabs found at the site and neglecting to mention not only the giraffes and elephants, but the many bovids, horses, monkeys and baboons, hyenas, cats, rhinos, rodents, and pigs also found there. We know that shorelines are good places to get fossilized, and that those animals which frequent such places are fossilized more often than others. This is also evidence which is ignored by AAT/H proponents, since, if our early ancestors were frequenting shorelines, we would expect to find far more than the relatively few we do find. Instead we find the sort of numbers we might reasonably expect from an animal which was spending most of its time elsewhere.

But it seems Morgan would have it that if you die in hospital; you must have spent most of your life there.

On page 30 Morgan tells us that crocodiles are just friendly, fish-eating fellows; which would come as a surprise to people who actually study crocodiles, like Charles A. Ross, croc specialist at the Smithsonian, who describes the Nile crocodile's diet: "Very large animals eat antelope, zebra, warthogs, large domestic animals, and humans". Not to mention the 100-300 people killed by them each year in Africa. More info on the "Predators" page on this site. Morgan also makes the mistake, which could have been easily corrected by minimal study, of assuming the smaller species of crocodiles in the region of Hadar today are the very same types seen there, in a very different environment 6-8 million years ago.

For Morgan, saying that predation by crocs is a massive problem for the AAT/H is "something on a par with saying that they could not have lived on the grasslands because they would all have been eaten by lions." But as you'll see on the "Predators" page, we have a model for savannah-living primates similar to hominids being able to survive in spite of predators -- savannah-living chimps. So we know it could be done there, by animals which are similar to our ancestors in size, intelligence, and birth rate (ie. population replacement rate). What we don't see is any such creature -- of any species -- in an aquatic environment. Anywhere.

Page 31 presents us with a beautiful, and unfortunately typical, example of the argument style termed "special pleading". Morgan attempts to deal with one of the 5 unanswered questions I mention on my page "Relevant Questions for the Aquatic Ape Theory". These are about actual aquatic traits of mammals (as opposed to the many claimed but not really "aquatic" traits the AAT/H uses) and I ask, why don't we exhibit *any* of them? The one she's wrestling with is about aquatic mammals and the length of their legs; they have shorter legs (or incredibly modified legs, like seals and whales) compared to their terrestrial relatives. Our australopithecine ancestors had legs and arms which were the same length as our terrestrial relatives. Why didn't they change? Morgan's answer is that there wasn't time in this "brief" period (Morgan: "a matter of two or three million years") for skeletal changes. She couples this with a strawman (again) suggesting the question refers only to seals and whales, when it's actually all aquatic mammals.

pg. 31: "As for the conformation of the skeleton of the earliest hominids, it has been claimed that if they were or had been passing through an aquatic phase their fossil remains would show unmistakable signs of it,

as do the skeletons of seals and dolphins; their legs would have become shorter instead of growing longer and their arms would have begun to turn into flippers."

Now if any critics had actually claimed what Morgan says they (we) did, I'd have to say they're out to lunch. But of course they (ok, it was me, actually) simply said what I did above. Morgan decided not to tackle that very reasonable question, instead making a phony version of it which she *could* successfully attack. That's just the strawman part; the special pleading part is more serious.

You see, Morgan claims there wasn't enough time to change the skeletal features of hominids during her purported aquatic period yet, at the same time, she insists that there was a massive change to the skeleton, specifically the pelvis during that time and due to this aquatic period. This means that she says there *was* enough time for these changes, but only for *her* changes, and *not* for the ones that are ubiquitous aquatic traits in mammals. This is the essence of special pleading; here Morgan uses a double standard by insisting that her theory's equally great (or greater) skeletal changes could take place even though the counterargument's skeletal changes could not. (She often does this regarding other features as well).

Oh, then she starts floundering and suggests that (even though aquatic mammals all exhibit this trait vis a vis leg length) we simply ignored our mammalian heritage and changed like (some) birds instead. Let's get this straight; you don't just get to go down to the store, pick out an evolutionary trait from a catalog of any animal feature, and install it, no matter how handy you think it might be. Phylogeny is, after all, the central idea behind evolution. Real evolution, that is, not Morgan's version.

Chapter 4: Morgan suffers some confusion in this chapter. First she compares the energetics of running in humans with that of unrelated quadrupeds, showing what might be expected for a species which is particularly adapted for walking; we use more energy than they do while running. Then she gets closer to home, and even more muddled.

She gets out her strawman kit again, with her line: "Since 1970 one of the goals of anthropologists has been to erase the image of human bipedalism as recklessly profligate of energy." Actually, researchers simply asked the question: We think bipedalism would have been more energy wasting than quadrupedalism for our earliest ancestors, but we don't have any data one way or the other; shouldn't we do some experiments? Rather than the conspiracy Morgan sees, this is a pretty sensible way to do some science. Why does Morgan find this so suspicious? Perhaps

because it contradicts her thesis that bipedalism was more wasteful. Here's what they found.

First, even for chimpanzees and capuchin monkeys (natural quadrupeds), running on two legs is no more wasteful of energy than running on four. Second, in another study, walking by humans today is *far* more efficient than quadrupedal walking by chimps. So, contrary to Morgan's long-held beliefs, walking bipedally was *not* likely to have been a disadvantage for our earliest ancestors. Yet the thrust of the AAT/H's argument regarding bipedalism has been that it could not occur without the support of water. And Morgan has argued this for more than a quarter century despite these facts being available in published form, easily accessible to any researcher, since 1973 and 1980, respectively. That's 4 Morgan AAT/H books since 1973, 3 since 1980, and only now has she bothered to look at the research on the subject. But hey, she dismisses it anyway, so I guess it wouldn't have made any difference to her theory-building. But why should anyone be expected to accept her theory if its premises are contradicted by currently available facts?

In chapter 5 she outlines, in somewhat dismissive style, a few of the reasons scientists suggest we might have found bipedalism advantageous. The major problem she's always seen with this is that there are so many reasons, rather than the one she suggests. She seems to feel that having one reason only makes one more likely to do something than having a dozen. Think about this: imagine you have two possible courses of action; there is one reason in favor of the first course of action and a dozen reasons in favor of the second. Which course will you more likely take? Personally, I find that the more reasons I have to do something, the more likely I do it. Morgan apparently marches to a different drummer.

On page 66, Morgan makes (as she often has before) the bogus claim that proboscis monkeys, which swim well and fairly often, use bipedalism more often than other primates (all primates use bipedalism occasionally) and often walk bipedally as "merely an alternative locomotor mode of getting from A to B." This is false. Morgan's source for overturning years of observations by primatologists? A few seconds of film showing some proboscis monkeys walking upright.

Then on page 70 she once again shows a remarkable blindness of vision regarding evidence, as she gleefully describes an example of an extant ape walking bipedally in water, "not for display behaviour, or sentinel behaviour, nor for reaching up for food on an overhead branch. It was walking bipedally in order to get from A to B." Really?

Well, no, not really. I too was fascinated to see these *National Geographic* pictures of gorillas wading, as they often do in that particular area, while feeding on plants. My fascination was more to the total situation, but I admit I also found it interesting that they exhibit exactly the sorts of locomotor behavior as gorillas who aren't wading: walking quadrupedally and standing for display behavior and to reach food. And that photo? The gorilla is standing stationary on two feet, staring directly at the cameraman (who was on a platform) and its arms are clearly wet up to a point about midway between the elbow and shoulder. In other words, we have a gorilla who was wading quadrupedally, saw someone, and stood up -- typical gorilla display behavior. Why didn't Morgan see this? Because she didn't want to. Nobody really *wants* to find evidence that upsets their own theories, but anyone worth their salt accepts it when they do, and anyone who doesn't can't very well expect their theory to be accepted as valid.

Morgan opens Chapter 7 with a quote from Darwin which she misrepresents in much the same way she did to him in *The Scars of Evolution*. The quote is "No one supposes that the nakedness of the skin is of any direct advantage to man." This quote is from Darwin's 1874 (1871 for the 1st edition) *Descent of Man, and Selection in relation to Sex*. Morgan leaves off the last part of the title, but a lot of people in academia do that too, even though they shouldn't. No marks off for that then :-).

Big marks off for not letting Darwin say what he actually said. First let *me* say that everyone should read Darwin. He was a terrific writer as well as thinker. His work is really readable and still thought-provoking; it's no wonder his books were best-sellers. He is one of our best science writers ever. You can get his books at any bookstore for dirt cheap, and if that's not cheap enough, you can download them for free from Project Gutenberg (there's a link on the links page). When you read Darwin you'll find that a common Darwin technique is the use of a setup for a point he's about to make. He opens with a passage in which he plays Devil's advocate, citing the supposed difficulty of explaining such a feature. Then he explains it -- in near-excruciating detail. Dishonest quoters, such as creationists, often use the first part of one of Darwin's setups as if it were the complete thought. They will quote only the opening passage and leave out the meat (creationist Duane Gish is reported to have excused this by saying, "After all, you have to stop quoting somewhere.")

Morgan uses this technique here, quoting the setup and ignoring the meat. In the *complete* sentence Morgan quotes *part* of, Darwin says: "No one supposes that the nakedness of the skin is any direct advantage to man; his body therefore cannot have been divested of hair through natural selection." He uses this as a setup for

his next paragraph, which begins: "The absence of hair on the body is to a certain extent a secondary sexual character; for in all parts of the world women are less hairy than men. Therefore we may reasonably suspect that this character has been gained through sexual selection." Then he goes on to provide 7 long paragraphs of info to support his contention. Note too that Darwin's statement suggests that adaptionist scenarios -- like the AAT/H -- do not explain the human hair patterns; this seems to escape Morgan's notice.

On the next page Morgan sarcastically derides the very idea of using the concept of sexual selection to explain the pattern of hair seen in humans, lumping it in with Wood Jones' "numerous quaint theories". As we've also seen in her earlier books, using sexual selection to explain these patterns really bothers Morgan. Why? Because using it makes perfect sense.

Darwin came up with the idea of sexual selection to explain a certain type of trait, and this idea has been accepted because, like his concept of natural selection, it makes sense and has been confirmed by many observations over the course of more than a century. Traits which are due to sexual selection have a couple things in common: they appear or change radically at puberty and they differ between the sexes. This pattern is true of several human traits which Morgan and other AAT/H proponents claim are aquatic adaptations (natural selection) rather than due to sexual selection. These are the pattern of hair growth, the sebaceous glands (and complimenting this the scent receptors), and the pattern of fat in humans. In aquatic mammals these features are the way they are by the end of infancy, and change little after that. If you've gone through puberty, or know someone who has, you know this isn't the way it is in humans. It's not easy to ignore this obvious fact, and it's so easily evident to anyone who looks at the people around them or themselves, that Morgan's only recourse is to try to bury the concept of sexual selection. She uses the shovel of sarcasm, which can be used, like any shovel, either to reveal or to hide.

So once you admit that -- as even a glance around you would show -- human hair patterns differ radically between the sexes and also change radically at puberty, you must be led to the same conclusion as Darwin was:

"The view which seems to me the most probable is that man, or rather primarily woman, became divested of hair for ornamental purposes, as we shall see under Sexual Selection; and, according to this belief, it is not surprising that man should differ so greatly in hairiness from all other Primates, for characters, gained through sexual selection, often differ to an extraordinary degree in closely related forms."

Charles Darwin, 1871, *Descent of Man, and Selection in relation to Sex*.
(above from Project Gutenberg's copy of the 1874 2nd ed.)

Oh yes, on page 72 she offers a mea culpa about her previous quoting of Frederick Wood Jones about hair loss, or rather the lack of it, in humans. She says quite correctly "I did not know its origin" but she completely misses the point about the fact that what she did wrong when she quoted him by proxy (she got the quote from Desmond Morris's *The Naked Ape*), was that she simply made up the situation whereby Morris got the quote. I guess she thought it sounded good to say it was said during a TV show rather than a 1929 book. Since she apparently quoted it in order to deride the thought, perhaps she felt the made-up TV reference made it seem less authoritative. Odd for a professional TV writer, but the world is sometimes an odd place. (Shame she never offers any thanks to the person who pointed out the actual source of Wood Jones' quote. :-)

So Morgan inadvertently teaches us another point to remember about science if you want to be taken seriously: don't just make stuff up.

You want to read about the creationist style I referred to in quoting Darwin? Just click [here](#).

In chapter 8 one immediately sees Morgan having trouble with the evolutionary concept of "convergence" (there's a definition on the "What is the Aquatic Ape Theory?" page). She's attempting to find a way around the fact that convergent features which are due to environment are generally found as a *set of traits* shared by relatively unrelated animals. The reason she finds this a problem is that there are ubiquitous traits among aquatic mammals, yet none of them are found in hominids; while her purported aquatic traits are either actually nonexistent, not shared with aquatic mammals, or unique to humans. Talking about arctic animals and their white fur or feathers, she says "But the Arctic fox and the Arctic hare and the others have converged in respect of one single trait only." (pg. 79). But this is not true, as even minimal study would have told her. In fact, it was pointed out to her online before this book came out, as I described on my ["General Problems with the AAT/H"](#) page. Besides the white coats, these convergent arctic traits include feet covered by fur or feathers, shorter extremities, and small ears, all of which help to contain heat rather than radiate it. (We see the converse in the arctic fox's relative the Fennec, a desert fox with enormous ears which act as radiators.)

These arctic traits are well known and oft-discussed; it would be easy for anyone who is doing even the least amount of research into convergent evolution to report

this accurately. That Morgan did not suggests either a deliberate misrepresentation or very shoddy research; neither encourages one to have confidence in her work.

Pg 79: She uses the earlier studies of Scholander et al., conducted using the skin and fat of dead animals, to claim that fat is a better insulator than fur for these arctic species. But later studies with living animals, which were conducted well before Morgan began her AAT/H work, showed that the reverse is true in living animals.

In the next few pages she spends some time trying to convince us that elephants, pigs, and rhinos are "in some degree aquatic". From her explanation this apparently means they can swim pretty well (and many times AAT/H proponents treat such habits as wallowing in mud as "aquatic"). In this sense virtually all mammals could perhaps be said to be "in some degree aquatic." Then she mangles the problem of size and thermoregulation. Large animals have less surface area compared to their volume than small ones, and consequently, they lose heat less rapidly. This causes a build up of heat and is correlated with less body hair, except for those large mammals in cold areas (think woolly mammoth and rhino of the ice ages versus their present-day tropical cousins). Morgan, on page 85, insists this can't be so because bison have hair and pigs don't. She ignores the facts that bison are not tropical mammals (been to Alberta or South Dakota in the winter lately?) and that non-tropical non-domestic pigs (and some tropical ones) have plenty of hair.

Later that page (pg. 85) Morgan makes an even more bizarre statement about Galapagos tortoises and their shells; she apparently doesn't understand the concept of tortoise shells as protection. I wouldn't have believed that one if I hadn't read it myself.

Chapter 9 offers us some misinformation on fat. Page 88 mentions Rose Frisch's statements about female fat requirements for menstruation (and therefore reproduction) but not the research that showed her percentage requirements to be too high. Morgan also repeats the statement about humans having ten times as many adipocytes (fat cells) as would be expected in an average mammal of similar size, which she gets from Caroline Pond's 1987 article which points out that this feature of relatively small and numerous adipocytes, is common to humans, fin whales, hedgehogs, monkeys, and badgers, and not rats. Why did Pond mention rats? Because Pond was pointing out that rats, with their relatively large and few adipocytes, are not appropriate models for fat studies in humans, and that other mammals are a better fit. Pond also points out, in the same article, that captive

monkeys which are not as lean as wild monkeys also have a similar ("ten times the..") number of adipocytes, but Morgan either misses or ignores this info, as she has since she first cited Pond's statement over a decade ago.

On page 91 Morgan again takes one of those ugly facts Huxley talked about 100 years ago ("a beautiful theory destroyed by an ugly fact") and attempts to bury it with her sarcasm shovel. It's about hippos this time, and the ugly fact is that these aquatic animals aren't very fatty. That is, they're plenty round, but they don't have very much fat -- the AAT/H says they should, but they don't. Naturally, Morgan doesn't like that fact, so a quick sidestep is in order; she accomplishes this by saying (pg. 91) "It is true that one article in the *New Scientist* stated in respect of the hippopotamus that contrary to appearances '...it is certainly not fat'. But this statement was made by a man who was trying to promote the marketing of hippopotamus meat. Perhaps what he meant was that a hippo steak would be less marbled with fat and contain less cholesterol than a beef steak."

Actually, what the fellow meant was the fact that a hippo just plain isn't very fat. Morgan tries to imply they aren't lean by talking about the thickness of the layer of fat under their skin, but seems to be unaware that a very large animal has very large dimensions in an absolute sense even when those dimensions are not particularly large in a relative sense. This is such an easy point to understand that I feel reluctant to provide an example for fear I'll seem to be talking down to my audience, but since Oxford grad Morgan has a problem understanding this, maybe others will too. So think about, say, the Sumatran rhino -- the one rhino which spends a fair amount of time in water, also known as the hairy rhino (darn those facts, eh, Elaine?) -- which is a very small rhino even though at around 800-1000 kg, it's a very large animal. In the absolute sense, it's big; in the relative sense, compared to its 3500 kg relatives, it's small.

Starting with page 96, Morgan attempts to come to grips with Caroline Pond (I keep wanting to refer to her according to her specialty, but "expert on fat" seems unwieldy and "fat expert" doesn't really work either). Morgan has attempted in the past to claim Pond agreed with her on the AAT/H, but somehow that seems unlikely when the very first article of Pond's that Morgan cited contained the Pond-authored sidebar "Not an aquatic ape--just an exceptionally fat mammal". Morgan then tried a new tack, claiming that though Pond said that human fat characteristics weren't due to an aquatic stage, she had no idea what it was due to, but since Pond has been saying (even in that 1987 article) that it's due to sexual selection, Morgan's technique there also rings false. Here in chapter 9 Morgan begins with a double-pronged approach to try to counter Pond's observation that human fat distribution is just like that seen in other primates and very unlike that of the aquatic mammals Morgan so wants it to be similar to.

First Morgan carefully builds some false context to make it seem that Pond is talking about something she isn't. Morgan says:

Pg. 96: "So, it is argued, we are not looking at a specifically human tendency for the adipose tissue to desert the internal depots and fly to the periphery: it is merely that 'An apparent shift in the *distribution* of adipose tissue arises as a direct consequence of an increase in its *abundance*.'"

"But the same thing applies to aquatic mammals like the seals."

The quote within the quote is Pond; Morgan's preceding sentence suggests that Morgan's insistence that humans have fat which is bonded to the skin and not to internal depots is a reality that Pond is explaining. But as Pond has explained many times, this AAT/H idea is not reality. The sentence after the quote attempts to obscure the fact that fat distribution in aquatic mammals, especially "like the seals", is radically different from humans. This is even more true when you look at fat distribution over the life history of aquatic mammals "like the seals" versus humans. There's more on this subject on the page "Fat and the AAT/H" so I won't repeat it here.

On to page 97, and Morgan is perplexed: "I find it hard to understand why she described the insulation hypothesis as 'a major tenet of the Aquatic Ape Theory'." Well, besides the references Pond provided for that statement, there's someone named Elaine Morgan who's used it as such in virtually everything she's written. Check out page 15 of Morgan's article in *The Aquatic Ape: Fact or Fiction?* from 1991; page 59 of her 1995 book, *The Descent of the Child*; pages 111-112 in her 1990 book, *The Scars of Evolution*; and it's virtually the entire argument of chapter 3 in her 1982 book *The Aquatic Ape*. (That's the problem with putting things in writing.)

Pages 99 and 100 see Morgan trying to kill the idea that human fat distribution is a sexually selected trait by pointing out that babies are fat. As she correctly points out, "epigamic adornments appear at puberty." She fails to see the reasons this isn't a valid argument (or maybe she does and just thinks her audience will buy it anyway). One is that during childhood we (both sexes) go through the leanest period of our lives, until puberty radically changes our fat quantity and distribution. The fat at puberty, and the differences between the sexes (has anyone besides Morgan *not* noticed this?) shows the classic signs of a sexually selected trait. The other is that the fat period of babyhood corresponds to the long postpartum developmental time in our species, which is unique. There's nothing else like it; not even close. And it's apropos to note that all aquatic mammals are the exact

opposite in this postpartum period, having babies which are quite advanced compared to similar terrestrial mammals, or which grow very quickly, or both.

Also on pg. 100, Morgan suggests that the fat differences between the sexes in humans was due to major differences in habits of AAT/H females vs. males, with females being far more aquatic. Interesting that she does so, since she also lists sebaceous glands as aquatic adaptations (when they too are undoubtedly sexually selected); the far greater incidence of sebaceous glands in male humans would then have to mean that males were far more aquatic than females. This sort of mistake is called *ad hoc* reasoning.

To end this chapter, Morgan takes another shot at misrepresenting Pond's position. I haven't checked out the quote Morgan uses in connection with this, but it looks like her Darwin-quoting technique... more later when I've checked it out. At any rate, the quote is about differences in fat between the sexes, and so is not on the subject Morgan says it is, which is "Why are humans so fat?" Morgan says Pond has no idea. The actual answer is, simply, because we can get away with it. This view is backed up by research in other animals vis a vis predators, and there's more about it on my "Fat and the AAT/H" page. Oh yes, one of the researchers who's shown this to be so? Caroline Pond.

Morgan really gets to flailing wildly in chapter 10, as she tries to salvage her claims that sweat and tears are aquatic adaptations, while (almost) admitting that her evidence was nonsense. She opens with a self-serving quote from S.J. Gould, "Error is the inevitable by-product of daring" without thinking that error is also the inevitable by-product of using falsehoods as evidence.

There's a lot about salt, sweat, and tears here on my site ("Tears and the AAT/H" and its subsection on Homeostasis, "Salt and the AAT/H", "Salt glands", and "Skin, sweat, and glands" are the pages); I won't repeat much here. Suffice it here to say that her past statements about salt excretion via sweat or tears violate basic principles of physiology; stuff that's well supported and well known, and which has been well supported and well known for long before she started writing about the AAT/H and decided that established basic physiology was an encumbrance her theory didn't need to conform to.

Page 115 implies that eccrine sweating is less efficient than apocrine sweating (sweating via eccrine glands is one of humankind's major claims to fame to date :-). Seriously, sweating through eccrine glands is why !Kung hunters could run down much faster apocrine-sweating animals like giraffes. We just keep going and going, while the giraffe doesn't, because our eccrine glands can keep going and

going, while their apocrine glands work rather nicely for about 20 minutes and then need a lengthy rest and recharge period. Hare and tortoise stuff.

On pages 115-116 Morgan tries again, despite the long-established facts of basic physiology, to have sweat be a salt excretor. It can't work; in fact, researchers on the subject have commented that sweat glands seem constructed so as to save salt. Sure, some escapes, but we always, always lose far more water than salt. This is explained in more detail in the Homeostasis subsection of the "Tears and..." page.

On the same page Morgan defends her earlier salt claims by saying, "It was all drawn from respectable academic sources." She's trying to defend her validity as a researcher by saying hey, I was just repeating what the experts said. This is not true; check my site's pages I mentioned above for examples. Her sweat salt claims were false, they did not say what the experts said at the time, including those she cited, and her claims violated known basic physiological principles. There's really no defense for that.

On page 119 she says that "The credibility of the AAT would indeed be weakened if..." and goes on to offer several things that would do so; the second and third are "...or if it had not been based on scientific data that had appeared valid at the time; or if I had continued to believe in it when the balance of the accumulating evidence clearly swung against it;..." Both these things are true, and the evidence on salt, which she distorted from the start, is just one of the examples of that fact.

Chapter 11: Oh no, it's more *ad hoc* reasoning, about the larynx this time. The descended larynx of humans is apparently unique only in that the hyoid bone descends with it, and is not at all like the descended larynx of those few aquatic mammals who have it, but that doesn't stop Morgan. The most *ad hoc* part of her larynx reasoning can be seen in the fact that it isn't descended for the first 2-5 years of our lives, unlike the larynxes of aquatic mammals which have descended larynxes. Remember that our baby fat is supposed to be an aquatic adaptation. According to AAT/H reasoning, this means that our larynx says our first few years weren't aquatic, but our fat says it was.

~~Also, fossil evidence shows the larynx wasn't descended until millions of years after the supposed aquatic phase. Maybe it was a delayed reaction. I do that sometimes; someone says "boo" and I jump several million years later.~~

I struck through the preceding paragraph of text rather than simply replace it because I didn't want to pretend my mistake hadn't been made. At the time I wrote it, I really should've known better, because by that time there was newer and quite

different information available. And in fact Morgan should've used that info as well. It turns out the descended larynx isn't at all unique to humans among terrestrial animals -- older research had been done on dead rather than living animals, and newer techniques allowed researchers to discover that various animals, including chimpanzees and various deer species (red deer, wapiti, and fallow deer at least) have descended larynxes, and many other animals have larynxes that descend when vocalizing -- in fact, it seems to be a primitive rather than a derived trait. There's more about this on my [Breath-holding, the diving reflex, the larynx, and the AAT/H](#) page.

We enter chapter 12; on page 138 Morgan points out that apes, like other quadrupeds, have little or no motor control over the muscles of the diaphragm which help us when we talk. Those who study this subject tell us this was overcome due to our erect posture and bipedal locomotion; the muscles which otherwise would be constantly in use for locomotion of forelegs were freed up, and this turned out to be a valuable side benefit for vocalization. ~~Perhaps this is why our larynx, also useful in vocalization, didn't descend for some millions of years after we started walking upright.~~

More strikethrough on the larynx; from the newer research it seems likely -- extremely likely -- that the larynx descending was not a late trait, but probably a primitive trait; from comparisons with chimpanzees, however, it seems the descending of the hyoid bone (which helps in forming sounds for more effective communication) was a later trait, probably as part of the suite of communication helpers that included the improved breathing control that was a side benefit of bipedalism. (Of course Morgan will have none of that, as we saw last chapter.)

On pg. 138 Morgan repeats a mistake she's made before, and which she's been corrected about. The details are on my page "Can AAT/H proponents research be trusted?". When she talks about aquatic diving mammals which are trained to dive on command, she says "the amount of breath it takes before the dive varies according to what the depth of the dive is *going to be*." However, this study she's referring to doesn't mention breathing at all; it's about bradycardia, the slowing of the heart when diving.

Swimming baby alert on page 144! See my page entitled, appropriately enough, "The 'Swimming Babies' reference" for info on this universal mammalian feature presented by AAT/H proponents as a unique human/aquatic feature. Interesting to note that since this book came out in 1997, she was likely working on it at the very time she indignantly protested that her 1994 book had no mention of the swimming babies info:

"In The Scars of Evolution you will seek in vain for a mention of hair tracts or swimming babies or the direction of the nostrils.

Elaine Morgan, sci.anthropology.paleo on 13 Jul 1995

I guess she was saving it for this book.

Chapter 13 is a grab bag of goodies. On pg. 151 Morgan informs us that hymens are "common among aquatic mammals"; while toothed whales, seals, and sirenia have hymens, Morgan neglects to mention a batch of terrestrial mammals with hymens, including humans and lemurs, guinea pigs, mole rats, hyenas, horses, llamas, elephants, rats, horses, and some species of galago.

On pages 152-153, Morgan muses about the menstrual cycle, wondering why its about the same as the lunar cycle. Well, it's the same as the oestrus cycle of chimps, so why the amazement that we so closely resemble our nearest relatives? It should also be noted that although the lunar cycle is extremely regular:

The occurrence of menstruation is an irregular event in the sense that women do not have a constant cycle length. In no age group do 28-day cycles occur more than 16 per cent of the time, and further, any given woman menstruates at her mean cycle length on only 16 per cent of occasions."

E.W. Wilson, and P.I.C. Rennie, 1976, *The Menstrual Cycle*, pg. 36

Morgan is attempting to draw a correspondence between lunar tides and menstrual cycles, but to do so she simply ignores the fact that it doesn't really correspond at all. (I'm struck by the irony of being male and having to explain menstruation to a woman. Thank god for books.)

More false facts on pp. 153-154: "Sebum is an oily fluid whose only known function in mammals is waterproofing the hair and skin." Sebum is the product of sebaceous glands, and its primary function in most mammals with sebaceous glands is for scent (think pheromones), although seals have large sebaceous glands which aid in keeping the skin pliable (check out the page on "Skin, sweat, and glands" for info on seal skin and seal sweat -- amaze your friends with arcane facts about seal sweat!). How can you tell if an animal's sebaceous glands are primarily a sexually selected feature used for scent? By now you probably see the pattern -- if it's not sexually selected but due to aquaticness, it's active and functioning when the creatures hit the water; if it's sexually selected, it becomes active and functioning at puberty, and probably differs between the sexes. In seals these glands are

plentiful and workable by the first few months, when they start swimming, and they are pretty much the same in males and females. In humans these glands are incredibly scarce until puberty and those few that do exist don't work much at all until puberty; plus they are far more numerous in males, while females have better scent receptors. That screams sexual selection... it's a classic example of that type of feature really.

Morgan suggests, on pg. 157, that in humans the direction of the hair on the chest is good for water flow around the body if you are "performing some approximation of the breaststroke"; that would be an approximation which included keeping the head (and beard remember) completely out of the water and the arms held down along the sides. I'd like to see someone actually do that. I couldn't.

Pages 158-9 offers a pathological condition often -- but not always -- due to exposure to water (ear exostoses) as evidence of an aquatic past. She's done something like this before with an early *Homo* with signs of vitamin A poisoning (you'll see this is "Relevant Questions for the AAT/H"). The problem is, if we were adapted to an aquatic existence, why wouldn't this damaging susceptibility to an ailment -- which causes ear infections and can rob us of hearing -- have been weeded out through natural selection? The fact that it exists as a problem for us is evidence *against* an aquatic past not for it. This, by the way, is another classic logical fallacy, called "Irrelevant Conclusion" (also called "*Ignoratio Elenchi*").

Pg 166 offers the idea that differences in the percentages of haemoglobin and red blood cells in blood of marine mammals and apes and humans constitutes evidence for the AAT/H. Morgan, following the AAT/H proponent she got the info from, mentions the numbers for apes and humans but not for marine mammals. Wonder why? Could it be because those numbers make the aquatic mammals seem just a little too different? For instance, about 60% higher percentage of haemoglobin for seals as opposed to humans. And that's just the tip of the iceberg regarding these blood differences. Marine mammals, for instance, also have about *twice* the blood volume per kilogram as humans, and *ten times* as much myoglobin in its muscles, which is a protein which allows these mammals to store oxygen in their muscles for diving. They do this because they exhale when they dive, the opposite of humans. Morgan's AAT/H source, by the way, implies that these other facts (marine mammals vs. humans) aren't known, and that this is why he didn't provide them. Well, they are known, and easily found at that (check out an encyclopedia for instance). Why didn't he provide these facts that damage his case? Why didn't Morgan? Not mentioning available contrary evidence in this way is yet another logical fallacy, the "Fallacy of Exclusion". No one *wants* to mention contrary evidence, but in science you have to -- if you want to be taken seriously.

Finally (I heard that sigh!) we get to Morgan's last chapter, which is a page-long summary. How could you say anything incorrect in that small space? Well, don't underestimate Morgan, she's a trouper:

About the AAT/H, she says, "The answers it offers are speculative, but no more so than those of any other available model." (pg. 176).

I beg to differ. No matter what you call it -- theory, hypothesis, or model -- when it's built on "false facts" and has so many holes that a first reading can find as many as I have above, it's more than speculative. It's way past speculative and heading toward the realm of pseudoscience. If AAT/H proponents want to stay out of that realm, they need to heed the GIGO maxim, "Garbage in, garbage out". Unfortunately, if they do that, their "evidence" disappears.

Having been privy to the arguments on sci.anthropology.paleo in the mid 1990s, I can see that Morgan was reading and thinking about what was said. Many of the arguments she makes in *The Aquatic Ape Hypothesis* are reactions to critiques raised there -- even the title itself and giving page numbers for quotes. These points and many others were raised there, especially by Phil Nichols, Alex Duncan, and me (it would've been nice if Morgan had given us a little acknowledgment). However, the point is that Morgan could have taken these critiques to heart, because as that Carl Sagan quote I have on the first page says, "Valid criticism does you a favor". She could've used the information we dug up to *try* to make a more valid theory, but instead chose to just try to explain away some things, shovel over others, and offer many of the same old discredited arguments and "false facts" in a book which is superficially constructed to seem more scientific. Sadly, rather than create a work of science, she chose to make a facsimile of one.

Alister Hardy's original "Aquatic ape theory"

I hadn't actually intended to do more than a passing critique of Alister Hardy's original theory. But lately online there's been a fair bit of parrying valid arguments by falsely claiming that the critic's point can only be made by stretching the AAT into some parody of itself; and this parrying is often being specifically done by holding up Hardy's original version as being a "mild", "modest", "boringly obvious" version of the AAT and that "no serious objection to it at all" could be made. The claim is made that Hardy did no more than argue "that they were semiaquatic as is seen in many other extant human populations." and that Hardy's theory was merely a description of "how a sea-side life (beach-combing, wading, swimming, collecting coconuts, shellfish, turtles & turtle eggs, bird eggs, crabs, seaweeds etc.) explains many human traits". They say that the arguments against the AAT are only possible because the critics "amplify it to such an extreme point that it becomes absurd. If they take it as it was originally intended it is simply irrefutable."

Well, I like a challenge, so let's see if that's so. These folks get away with this because they assume that the critics haven't read Hardy's original articles and they're generally right about that. (Since those critics aren't critiquing Hardy's work, but the later versions by other proponents, there's actually no need for them to be reading Hardy.) I have read Hardy's work, and as I said, I like a challenge, so let's dive into Hardy's "irrefutable" version of the AAT.

Alister Hardy

Sir Alister Hardy was a marine biologist who specialized in the study of plankton -- and don't even think of laughing; he got knighted for it. Plankton is one of the basic components of the marine food chain, and his work was very important to the British fishing industry, not to mention everybody else's. Among his more important work, he invented a device in 1925 to better record plankton levels and map out the distribution of different varieties. This was called the Continuous Plankton Recorder, which was towed behind ships, and in 1929 he designed a somewhat smaller version of the CPR which could be towed behind merchant ships, getting more and better information from a much wider area than ever before. This device, in essentially the same form, is still used in ocean research today, over 75 years later. Pretty cool.

He had a long-time interest in spirituality, which was boosted by his experiences in World War I, which certainly affected a lot of people who saw, and survived, the carnage of that war. He was interested in the idea of trying to study spirituality with eye toward reconciling it with a scientific point of view. His idea of spirituality included not only his Christian (Unitarian) religion, but also telepathy and thought transference, and in 1969 he started the Alister Hardy Trust as "a focal point for people interested in the nature and study of spiritual, religious and psychic experiences". He suggested that telepathy had likely influenced evolution, including human evolution. He was also a president of the Society for Psychical Research, which has had a long and unfortunate history of testing "psychic" frauds and pronouncing them to be genuine. These things don't mean that his aquatic ape theory couldn't be a good theory -- he certainly had done lots of very good work as a scientist -- but it does indicate that he wasn't adverse to tripping over the fuzzier edges that mark the border between science and pseudoscience. And I'm not trying to tar his AAT version by association; just providing some information on the man, although it's interesting that when AAT proponents provide a bio of Sir Alister, they generally skip his spirituality work, even though it was a lifelong interest for him and consumed much of his retirement years. There's a long list of scientists who've done terrific work in one field while being pretty nutty in others, especially in

retirement when they can indulge their non-work related passions (William Shockley is a classic example, as is, on a more benign note, John Lilly). Hardy was an interesting guy, to say the least.

Critique of Hardy's version of the AAT

So back to 1960, when he gave a talk at the British Sub-Aqua Club (a scuba diving club) and a month later published in *New Scientist* a short article on that talk called "Was Man More Aquatic in the Past?" He subsequently appeared on a radio show whose transcript was then published (1960 again) and wrote another short article on the idea in 1977. The first article is reprinted in Elaine Morgan's 1982 book, *The Aquatic Ape*, along with excerpts of the other two articles showing those bits that differ from the original or add more information. It's an easy to find source so you can check out what he said as well as checking up on me to see if I'm accurate about what he said.

There are some things that Hardy definitely did better than his successors; his science background didn't desert him completely. He gave a fairly specific timespan for the suggested aquatic period, as well as giving some idea of how many hours he thought these hominids would be in the water, and he recognized that they would have to be in neck-deep water much of the time for his theory to work as well as recognizing that aquatic predators exist. These not so insignificant details were largely overlooked or glossed over by later proponents of the theory. But he made a series of whopping mistakes in his statements, seemingly because he didn't do much, if any, actual research at the library.

These statements of Hardy's are taken from the 3 sources Morgan has in her 1982 book, pages 137-153.

Hardy: *"The extent to which sponge and pearl divers can hold their breath under water is perhaps another outcome of such past adaptation."*

The fact that certain people can, with a lot of training, do amazing things doesn't tell us too much about the run of the mill, untrained population. As a counterexample, experiments have found that untrained dogs can hold their breath longer than untrained humans, and untrained individuals are of course more representative of a population. Consider too that humans can run incredibly long distances at pretty rapid speeds (several times faster than the fastest swimmers), yet this is not evidence of lots of high speed distance running in our past. Humans

can do a lot of things that aren't directly connected to our ancient past -- withstand high G forces, catch and hit balls thrown at us at 100 mph or more, live in the zero G environment of space. It's just something that highly trained humans can do.

"It may be objected that children have to be taught to swim; but the same is true of young otters, and I should regard them as more aquatic than Man has been. Further, I have been told that babies put into water before they have learnt to walk will, in fact, go through the motions of swimming at once, but not after they have walked."

In excusing the fact that humans need to learn to swim, Hardy points out that otters also need to teach their young to swim, and then suggests that in his view, otters are "more aquatic than Man has been". Yet otters don't show the features supposedly seen in humans due, according to Hardy, to being in some degree aquatic. And this problem is not limited to otters; it's true of most aquatic mammals.

Hardy also brings up the "swimming babies" feature, without mentioning that this behavior was seen in all mammals tested, aquatic and terrestrial, and that this was mentioned in the same paper as the swimming babies info, and actually on the same page. But then he apparently didn't bother actually researching this point at all, as indicated by the phrase "I have been told that..." in his statement. And he never looked at it further, apparently, which is typical of his statements throughout. Not the way to put forward a scientific theory, but nevertheless a precedent for many later AAT efforts. Unfortunately.

Hardy starts an AAT perennial with this statement: *"Does the idea perhaps explain the satisfaction that so many people feel in going to the seaside, in bathing, and in indulging in various forms of aquatic sport? Does not the vogue of the aqua-lung indicate a latent urge in Man to swim below the surface?"*

This is the "race memory" idea, a common theme running through AAT writings. There is no reason to believe that it has any validity whatever. There was a guy, a psychologist I believe, back about 30 or so years ago, who tried to show that humans had lived on savannahs by pointing out how much people liked lawns. That was a stupid idea, and it was sensibly dismissed just as the AAT version should be. To see the absurdity of this sort of statement, try this version: "Does not the vogue of the hang-glider indicate a latent urge in Man to fly above the surface?" Yes, it does, there is no doubt that it does; we do have a desire to fly above the earth -- so does this provide even an iota of support for the idea that we went through a flying phase during our evolution? No, obviously not. Yet it's as sensible a statement as Hardy's statement about scuba diving, as well as the other "race memory" ideas he put forth, and just as meaningless as evidence of some phase in

our past. Humans have desires to do a great many things that don't indicate anything about our past except for our sense of wonder -- we want to swim beneath the surface for long periods, we want to fly, we want to travel faster than we, or any other animal, can run, fly, or swim, we even want to travel to other planets. We don't have "race memories".

"Whilst not invariably so, the loss of hair is a characteristic of a number of aquatic mammals, for example, the whales, the Sirenia (that is, the dugongs and manatees) and the hippopotamus. Aquatic animals which come out of the water in cold and temperate climates have retained their fur for warmth on land, as have the seals, otters, beavers, etc. Man has lost his hair all except on the head, that part of him sticking out of the water as he swims; such hair is possibly retained as a guard against the rays of the tropical sun, and its loss from the face of the female is, of course, the result of sexual selection."

"Whilst not invariably so" is right, although a bit coy about the reality of the situation; in fact loss of hair is highly unusual for aquatic and semi-aquatic mammals. Hardy tries to deal with this fact, or more correctly he tries to sidestep around it, but because he didn't face it honestly (or just didn't know the facts and didn't bother to look at a basic encyclopedia or something) he gets this all wrong too, with this statement: "Aquatic animals which come out of the water in cold and temperate climates have retained their fur for warmth on land, as have the seals, otters, beavers, etc." But the fact is that retaining their hair isn't just something restricted to those mammals in cold and temperate climates; actually almost all aquatic mammals in warm areas also retain all their hair.

"Man has lost his hair all except on the head" (sic) even though in the rest of the paragraph (not quoted here) he contradicts himself by pointing out a bit of reality and describes how the hairlessness of humans is more "apparent" than real. And at least he recognizes the likelihood of sexual selection in human hair patterns, as well as seeing the possible effect of neoteny.

Hardy goes on to mention hair tracts as, in his view, reducing drag -- but if the body hair is curly, as it often is in hairy people, does the direction of the hair tract actually reduce drag, since the hair is not lying flat along the body anyway? And then of course there's head hair and beards, which don't seem to fit this theory. Hardy suggests, as most AAT/H proponents have since, that head hair was retained to guard against sun. That in turn suggests that most of the day found these hominids in water up to their necks, yet he wants them in the water only part time. Also, when thinking about body hair and reducing drag in the water, consider the erector pili muscles in humans, which are what causes our hair, and that of many animals, to be able to "stand on end"; note that this feature is absent in seals and sea otters, so their hair can lie flat along the body to reduce drag. When AAT

proponents are confronted about this sort of thing, they usually say there wasn't time for that change (although we'll see that Hardy's scenario offered plenty of time) yet they also say there was plenty of time for other changes (sebaceous glands, for instance) to seal-like features. Why not this other incredibly useful change?

The graceful shape of Man-or woman! -- is most striking when compared with the clumsy form of the ape. All the curves of the human body have the beauty of a well-designed boat Man is indeed streamlined."

"Man is indeed streamlined." Oft said by AAT/H proponents since, but never any evidence, from Hardy or anyone else. And a casual look shows it isn't so. (I ask the reader to go into their bathroom or bedroom, someplace with a full length mirror, take off all your clothes and look at yourself from the front and the side. Do any of you have the sleek curves of a "well-designed boat"?) Morgan has offered as evidence a picture of a human in bathing suit and cap diving with her arms held before her versus a picture of a sitting chimpanzee. But a more direct comparison, of similar poses, shows two animals with shoulders which jut out, rather spoiling the streamlining. The motions we use when swimming upset this streamlining even more, as opposed to the very different types of motions seen in aquatic animals. And while whales and seals are actually streamlined via their fat deposits, those of humans stick out in the most inconvenient way when it comes to being streamlined. Breasts, buttocks, all that fat just makes our shapes a bit too lumpy to be streamlined. Hardy at least mentions fat in this regard, as a shaper, which puts him well ahead of the thinking of later AAT/H proponents, but the fat deposits of humans don't add to their "streamlining", but detract from it.

At least he sees that these hominids would have to be wading in deep rather than shallow water for it to support their weight, etc., which puts him far ahead of many AAT proponents now, who seem to want their hominid to be in shallow water at the edge, water which will somehow support them and somehow cause them to lose their body hair (well, some of it, in some people but not others, at some ages, but not other ages).

It seems likely that Man learnt his tool-making on the shore."

He sees hominids learning tool use "at the shore" not knowing that chimpanzees use tools in the wild; why wouldn't we be like our near relatives? He has a reasonable excuse for this mistake, because this wasn't often described at the time as wild chimp behavior (although there actually were reports of wild chimps using tools for 100 years before he wrote this), but then he goes on and has his hominids making sharpened stone tools, making fire, and then they're ready to be off to hunt "grazing game". Then, once ashore again, these hominids apparently simply forgot

all this profoundly useful stuff for millions of years. But of course this particular idea of his was simply a wild speculation without any foundation and with more than a few points against it, in that modified stone tools and fire weren't known until much much later than the period he proposes. Millions of years later.

My thesis is, of course, only a speculation-an hypothesis to be discussed and tested against further lines of evidence. Such ideas are useful only if they stimulate fresh inquiries which may bring us nearer to the truth."

At least he recognizes the difference between a speculative hypothesis and a theory, and knows that such things are not useful if the evidence is against it. You have to give him points for that. Unfortunately, he didn't really examine the evidence closely before he put forth his idea. He was, after all, originally just talking to a bunch of scuba divers, not intending to put this into print. But when the newspapers wrote about his talk and he wasn't happy with their characterizations, he wrote this up. Sadly, he never did examine the evidence very carefully, even some 17 years later when he wrote another article on it.

In another publication, this a transcript of a radio talk he gave later in 1960:

Several people have asked me why, if Man has had a long enough evolution in the water to produce such characters as loss of hair and subcutaneous fat, he has not also got webbed hands and feet. Regarding the development of hands, I am sure that selection would not favor such mutations, for his separated fingers would be of greater value in finding and dealing with marine food. But regarding the feet, the truth is that some people have their toes webbed but they do not like to talk about it!"

Hardy offers an answer to people who asked him why, if we were so acclimated to water for so many years, aren't our feet webbed. Actually, I don't see people necessarily having webbed toes if they were aquatic, but then look at the timespan he's suggesting (20 plus million years, as we'll see later). Some humans do have partial webbing between digits. This is due to a small change in development timing, because early in development the digits are fused together; when this persists after birth the technical name is syndactyly. Some fairly recent papers I've seen (really in passing; it's not an intense interest of mine) shows that this timing change is how bats' wings and ducks' feet got the way they are; it turns out to be a fairly simple genetic change rather than the huge change it was often thought to be. Because it is so simple a change it would be likely enough to have turned up fairly quickly in an semiaquatic environment.

But Hardy will have none of this; he also handwaves away the fact of its rarity and suggests that the real answer is that "some people have their toes webbed but just they do not like to talk about it". It may be that people with even the minor amount of webbing he's talking about don't like to talk about it, but the fact is that it's not very common in humans, although it's not *incredibly* rare (Wikipedia says "approximately one in 2,000 to 2,500 live births"). More importantly, far from being something unique to humans among primates, it's also found in many non-human primates, like gorillas, siamangs, gibbons, catarrhine monkeys (macaques, for instance), and some lemurs. In fact the the second part of the siamang's scientific name ([i]Symphalangus syndactylus[/i]) specifically refers to this because it's a defining characteristic of that ape.

The following statements are from his 1977 article in *Zenith*, which Elaine Morgan describes as "the magazine of the Oxford University Scientific Society, which is mainly an undergraduate concern" (and which AAT/H proponent Algis Kuliukas inexplicably insults and derides as a "student rag").

Hardy jumps in with the wild overstatement "Man alone became a carnivore" (among primates) -- overstatement in the extreme, but let's treat it as hyperbole and ignore it.

A mammal that has remarkable human-like fingers on its fore-limbs is the American raccoon which habitually sits by the edge of a stream with its hands in the water feeling about for crayfish or other prey on the bottom."

He compares our hands to raccoons, without apparently even thinking of comparing them to, say, the dozens of species of other primates.

Next he talks about predators, and at least in this article he recognizes that aquatic predators do exist (while other AAT accounts ignored the possibility) but assumes that humans were like herd animals that lived in extremely large groups like some seals or penguins, and that they would see predators -- sharks in this case -- before they struck; both ideas are contrary to reality.

"Now look at the remarkable stream-line shape of the human form in fig. B; how different from any other of the primates are the beautiful curves of the body helped incidentally by the layers of subcutaneous fat-they are like the curves of a boat, so loved by many men. The rounding of the human jaw, fig. C, unique among the primates, has always been a puzzle to anatomists: it is shaped like the jaws of a frog."

Again with the absurd idea that humans are "streamlined". And a new one; the human jaw shaped like that of a frog? Not at all. The frog jaw is quite wide and shallow, with typically a slight V to its shape. The human jaw is pretty close to being a nice semi-circle. The side view is even more different -- I really don't know what he was thinking, as this idea really seems to be grasping at straws. And the shape of the human jaw compared to apes, rather than being a great puzzle to anatomists, is an obvious result of the flattening of the face in humans when compared to apes.

"So after some twenty million years or more of living a semi-aquatic life -- I must make it clear that I do not suppose man spent more than perhaps five or six hours in the water at a time -- Homo aquaticus left the sea (or lake) a very different creature from when he first entered it. Now with a hairless body, subcutaneous fat giving him a shapely form, a knowledge of making and using tools, and, above all, the erect posture, he might well be called a new species of man: indeed the ancestor of Homo erectus. His feet have always been a compromise between swimming organs and those adapted for running. About this time, I imagine, in fashioning flints he saw the sparks fly which led him to make fires of dried seaweed and driftwood along the beach; he was now equipped to cook the fish he caught."

Again he has our earliest ancestors making fire and advanced tools, including now not only knives but spears, and now they're hunting porpoises, an amazingly swift creature adept at diving, after which we're ready to be off hunting "deer and antelope". Except that of course we didn't for millions of years, having apparently simply forgotten our advanced tool-making, hunting, and fire-making skills once we got out of the water.

And here he offers both a timespan and number of hours in water: 20 plus million years and 5-6 hours in the water at any time ("no more than half" his time in water). Note that this article was 1977, when it was accepted by all but the die-hards that hominids had split off less than 10 million years ago, and even the die-hards took it back only to 12-18 million years. So his "mild", "modest", "boringly obvious" version of the theory states that hominids were aquatic for several times longer than they have existed, and longer than even the most die-hard holdouts against the fossil and molecular evidence suggested at the time he said this. That sounds to me neither "mild" nor "modest". Nor, for that matter, like someone who had even so much as cracked a book to peer at the facts therein.

"Perhaps it was not only a shortage of food that sent man to the water in the first place, but also a means of escaping from powerful predators: possibly Homo aquaticus was only able to survive and evolve with the help of a number of small sandy or rocky islands stretching up the tropical coasts or margins of lakes where

he could live in large colonies, like those of seals or penguins, and where his only enemies were sharks and killer whales in the sea or crocodiles in lakes and rivers."

Hardy started a long history of AAT proponents suggesting water as a safe harbor, an escape from predators, yet at least he, unlike most others, realizes that there are aquatic predators, although he seems unaware that bull sharks inhabit fresh as well as salt water (and he's a marine biologist) and that Indo-Pacific crocodiles (and others) inhabit salt water.

"For thirty years I kept the idea to myself, always waiting for the fossil evidence which I felt must surely come."

And later he says, "I am still waiting for the fossil evidence" -- and 37 years later we're still waiting for it, apparently, despite the accumulation of much additional, and earlier, hominid fossil evidence in the intervening years.

"This would really clinch the matter, but now there has come another discovery which is almost as conclusive as the fossil evidence, or so I believe. It has been found experimentally that man has the remarkable adaptation which is found only among mammals and birds that dive under water. It is called the diving reflex and now solves the puzzle of how sponge and pearl divers can remain below so long."

So finally, in what he thinks is a clincher to his long-held theory, Hardy offers the diving reflex, but for something which he feels is a powerful vindication of his theory, he seems to have done astonishingly little research about it. He incorrectly says it is only found in humans and aquatic animals, even though it had been known for decades (since the late 1930s, I believe) that it was found in terrestrial animals as well. Frankly I'd think a marine biologist might be expected to know better, and that someone, especially a respected academic who is offering this as an ultimate vindication of his pet theory might have perhaps, say, cracked open a book and looked up this basic info about it.

"All this, of course, is only an hypothesis and valueless till put to the test. Speculation is the fuel of scientific progress; it drives forward to discovery only if it is continually being burnt in a fire of constructive criticism."

Would that those who followed him had taken this route instead of the methods they have used to "support" the AAT. Unfortunately, the "aquatic ape" turns out to be exceedingly flammable for a water-dweller, and even the slightest criticism burns it beyond hope of recovery.

So much for Hardy's original "mild", "modest", "irrefutable" version of the theory. It turns out not to be very mild or modest at all, what with it claiming that we were semi-aquatic for 20 plus million years when hominids have existed for somewhat less than half that time, and the claim it makes that during this 20 plus million year period we learned to make advanced stone tools (including knives and spears) and fire, hunt big game, including large (and very swift) porpoises, then promptly forgot all that as soon as our feet hit the shore. Not to mention it using completely phony "evidence" such as the claim that the diving reflex is found only in humans and aquatic animals -- known to be false for decades before Hardy made the claim -- and the "swimming babies" evidence -- not bothering to mention that this characteristic of very young human infants is also characteristic of all the mammalian infants tested, including terrestrial infants.

Etc., etc., etc., as the King says in "The King and I". Hardy's version of the theory turns out not to be "mild", and not to be "modest", and especially, not to be "irrefutable".

Sorry.

Here's the online statements of AAT proponents I referred to as characterizing Hardy's theory as somehow "modest" and "irrefutable".

Algis Kuliukas

"It just shows me that the only argument they have against the AAH is to amplify it to such an extreme point that it becomes absurd. If they take it as it was originally intended it is simply irrefutable."

"the question Hardy posed was merely 'was man more aquatic in the past?' It is people like you who refuse to consider it in the modest, boringly obvious, sense - because then you'd have to concede that there can be no serious objection to it at all."

"If you take the theory (sorry, hypothesis) to be what Hardy had in mind originally, it becomes so mild that all the objections raised to it so far disappear."

Marcel Williams

"Apparently, you've never read the original AAT paper, Alister Hardy's (April 1960) New Scientist article 'Was Man more Aquatic in the Past?'. He argued simply that a

primitive stock of apes was somehow forced to feed on some shallow tropical seashore for shell fish and other marine invertebrates and that they did so by wading-- bipedally-- in shallow water and by diving for benthic invertebrates in deeper waters."

"Hardy never argued that human ancestors were 'mermaids', he argued that they were semiaquatic as is seen in many other extant human populations."

Marc Verhaegen

"how a sea-side life - wading, swimming, collecting coconuts, shellfish, turtles & turtle eggs, bird eggs, crabs, seaweeds etc."

"A biased & irrelevant website; not 1 argument against Hardy's theory that a sea-side life (beach-combing, wading, swimming, collecting coconuts, shellfish, turtles & turtle eggs, bird eggs, crabs, seaweeds etc.) explains many human traits"

Marc Verhaegen

Beginning in the early 1980s a medical doctor (general practitioner) from Belgium, Marc Verhaegen, penned a number of articles on his version of the AAT/H. In his version, rather than a limited time as an aquatic or semi-aquatic animal, virtually all of hominid evolution is "aquatic" -- albeit without any meaningful explanation of what he means by "aquatic". This is that problem with "ZING!ability" I mentioned on my "What is the AAT/H?" page -- in Verhaegen's hands their "aquaticness" varies from feature to feature and critic to critic, in an ad hoc zig-zagging that makes criticism difficult -- when you point to one degree of water use, the creatures ZING! over to another degree of water use, ultimately being wherever he wants them to be at any given time to counter valid criticisms, no matter what inconsistencies this creates. You find this is virtually all AAT/H accounts after Hardy; the degree of "aquaticness" is, shall we say, awfully fluid.

Verhaegen also entertains various other idiosyncrasies, such as positing Neanderthal noses as snorkels (due to sea otter-like diving habits, according to him). The more basic problems with Verhaegen's work are the by now classic AAT/H environmental determinism, the also classic AAT/H special pleading regarding convergent evolution, the classic AAT/H strawman version of mainstream paleoanthropological theories (restricting them, inaccurately, to savannas, and not just any savannas, but the super-restricted AAT/H strawman version of savanna), and the -- yes, classic -- numerous false "facts".

Since I find that one relatively recent AAT/H proponent tactic is to claim that the only problems with any proponent's ideas or claims are those I've written down here, let me stress that the following are by no means the only false "facts" that Marc Verhaegen uses or has used -- in fact he uses so many that it's difficult, time consuming, and even distressing, to go after them all. So I'm offering a few as examples, and using these examples to point out how unreliable, at best, his scholarship is. And the sources I give for his statements are not the only places he's said these things; he tends to repeat himself, and cite himself, quite a lot. And I've also found a few quotes, from referees of a paper that Verhaegen co-wrote, to show that I'm hardly alone in recognizing this problem of his.

With that in mind, here are only a few of the false "facts" Verhaegen has used or introduced into the discussion:

He calls this one of the *"features that clearly contradict the savanna theory and/or favour the (semi)aquatic theory"*:

"a body core temperature lower than 38°C"

pg. 165, Verhaegen, "Aquatic Versus Savanna: Comparative and Paleo-Environmental Evidence", *Nutrition and Health*, 1993, Vol. 9, pp. 165-191 and

"In other words, humans have a normal temperature resembling that of sea mammals, lower than most terrestrial ones, and markedly lower than that of any active savannah species."

1991 "Human regulation of body temperature and water balance", in *The Aquatic Ape: Fact or Fiction?* Edited by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds. Souvenir Press: London.

And he repeats this in the sci.anthropology.paleo newsgroup on Feb 7 2003:

"humans have a normal temperature resembling that of sea mammals, lower than most terrestrial ones, and markedly lower than that of any active savannah species"

First note, on this argument as well as the next, Verhaegen demonstrates a common tactic in AAT/H accounts (in addition to basing them on false "facts"), one which he has embraced with gusto, not only using it over and over in his work but taking it to extremes perhaps unmatched by any other AAT/H proponent. First, he ignores the fact that hominids did actually learn to live even on the open arid savannas that AAT/H proponents typically use as their strawman, doing just fine

with those characteristics he claims can't be found in savanna animals. He also imagines that in doing so we should have become like very distantly related animals (antelope, hunting dogs, etc.) instead of our primate relatives. In doing so, he ignores the central element of evolutionary theory -- phylogeny, the relatedness of one living thing to another. Doing this is a bizarre element begun by Elaine Morgan and which has since become common to much of the AAT/H as put forth by virtually all its proponents. And finally, he ignores primates, especially those primates which live in the type of open arid savanna he uses as a strawman -- no doubt he does so because the characteristics he's claiming can't be found in savanna animals are found in those open savanna primates.

Our body temperatures are in fact very much like our primate relatives, including those which live in savannas. There's a lot of information on this on the ["Body Temperature and the AAT/H"](#) page. It is certainly not like distantly related savanna animals such as the ungulates Verhaegen thinks we should resemble, *and this is to be expected* -- why AAT/H proponents are continually surprised that we resemble other primates far more than antelope and other distantly related animals has always amazed and perplexed me.

He also calls this one of the *"features that clearly contradict the savanna theory and/or favour the (semi)aquatic theory"*:

"daily temperature fluctuations of less than 1°C"

pg. 166, Verhaegen, "Aquatic Versus Savanna: Comparative and Paleo-Environmental Evidence", *Nutrition and Health*, 1993, Vol. 9, pp. 165-191

Human body temperature actually typically varies 2-3 degrees during the day even without strenuous exercise; exercise makes it vary even more. As with the ear exostoses mentioned later, the obvious question here is how can a medical doctor be ignorant of the medical literature on such a well-known and basic characteristic about which accurate information is so readily available?

The antelopes that Verhaegen, in the article cited, thinks we should resemble have specialized adaptations to allow their body temperatures to rise quite high (compared to primates) while keeping their brains cool enough to survive. It should not be surprising that relatively unrelated animals in similar environments use different methods to accomplish the same goal (in this case staying cool enough to live) since this is extremely common. You do not simply go down to the "Adaptation Store" and pick out a feature you think sounds nifty -- evolution simply does not

work that way and this has been known for, literally, centuries. Well, not known to Verhaegen, it seems, but known certainly to any reasonably thoughtful person. :)

He also calls this one of the *"features that clearly contradict the savanna theory and/or favour the (semi)aquatic theory"*:

"ventro-ventral copulation"

pg. 166, Verhaegen, "Aquatic Versus Savanna: Comparative and Paleo-Environmental Evidence", *Nutrition and Health*, 1993, Vol. 9, pp. 165-191

Verhaegen here expects us to give up a trait we find in various non-human primates (seen in orangutans, black-handed spider monkeys, and occasionally in woolly spider monkeys and gorillas) because of a change in environment -- certainly this must be one of the most extreme examples of the AAT/H's environmental determinism. Again, it's amazing and perplexing that AAT/H proponents are continually baffled by our resemblance to our closest relatives.

He has claimed that seawater is potable to humans:

"Never heard of the Bombard experiments? French Navy IIRC. When one gradually shifts to drinking small frequent bits of seawater + eating fish, it's possible to survive for months."

Verhaegen, May 8, 2004 sci.anthropology.paleo

This is incredibly stupid, and dangerous -- it's completely false and if followed, would result in extreme ill-health and, if continued, death. How a medical doctor could believe such nonsense is incredible to me. The experiments of Dr. Alain Bombard, and the subsequent followup experiments by the French Navy, did not show what Verhaegen claims they did.

Dr. Bombard did an experiment where he drifted in a raft across the Atlantic, supposedly without any rations being used other than things he got from the sea. I say supposedly because he did carry fresh water and provisions which he was not supposed to use, and it may be that he didn't, but Dr. Hannes Lindemann, who later did several similar experiments with more data reported, also reported that Bombard was reprovisioned at sea at least twice, with photos of one of these times being printed in a Dutch newspaper. Regardless, Bombard did definitely do something that was difficult at best and came through alive and in relatively good

health, although anemic and having lost 55 pounds in 65 days, and he did (and does) advocate the drinking of small amounts of seawater as an emergency measure. His voyage seems to very often be inaccurately reported (although generally not so inaccurately as Verhaegen did). His idea was to start off drinking seawater and getting fresh water temporarily (until he could collect rain) from crushing chunks of fish (which he would catch) in a press, and using a fine net to get plankton for Vitamin C, the major dietary necessity he couldn't get from fish. This is problematic, however, as Lindemann found that one couldn't get fresh water from fish in such a manner, not surprising since their bodily fluids are saline. Nevertheless, Bombard claimed he managed to do so and survived in this way for some days, although I don't find it clear just how many, not having his book before me. What I find mentioned online is that he drank seawater (in addition to his fish juice, apparently) for no more than 3 to 6 days at a time, but that rain didn't come for 23 days. This would suggest that he used other water before he got the rainwater. At any rate, after the rains started, his problem was not lack of fresh water, in fact, he joked that he might drown in it if the rain kept up, and after several such rains he had more than enough rainwater for his voyage.

The French Navy experiments suggested that small amounts of salt water could be drunk starting right away when the body was well hydrated, and that if you then drank fresh water after several days your body could then flush out excess salt. It must be stressed that they, and Dr. Bombard, considered this an emergency measure only, as they recognized that it's not a healthy thing to do -- their suggestion was simply that it could be used to stave off death at some cost to short-term health. They also recognized that seawater can only be used safely if used in small amounts and with fresh water being available soon to help dilute it and flush out the excess salt from the body. The dangers of salt buildup are loss of mental functions, general lethargy, and the danger of complete renal shutdown (which keeps the kidneys from getting rid of many of the body's toxins and results in a fairly rapid deterioration and death). It should be noted that Dr. Lindemann considered the drinking of seawater to be an extremely bad idea, and that no one (no one in their right mind) would suggest that seawater could be used as a water source (without desalination) for humans for more than an extremely brief period -- a few days at most -- Verhaegen's suggestion that it could be done for months is absurd.

Claims that supernumerary kidneys (extra kidneys) are common in humans:

"Humans have 10-12 papillae in every kidney, and frequently an extra (third or fourth) kidney."

pg. 169, Verhaegen, "Aquatic Versus Savanna: Comparative and Paleo-Environmental Evidence", *Nutrition and Health*, 1993, Vol. 9, pp. 165-191

Supernumerary kidneys are actually a rare occurrence, something well known and something one might reasonably expect a medical doctor to know even if he hadn't done any research on it (which of course he should have done before making the statement):

"Supernumerary kidneys

This is usually seen as a double kidney on one side and occurs in less than 1% of births." Klinik und Poliklinik für Urologie, Inselspital, Bern, Switzerland

"They are relatively rare..." The Encyclopaedia of Medical Imaging Volume IV 2

"Supernumerary kidneys

Third kidney is very rare and not to be confused with relatively-common, unilateral duplication of the renal pelvis." www.patient.co.uk

On exostoses in Neanderthals and *Homo erectus* skulls:

"The auditory canals of their skulls often show exostoses of a kind that is only seen in humans that swim almost daily in water colder than 18-20°" pg. 171, Verhaegen, "Aquatic Versus Savanna: Comparative and Paleo-Environmental Evidence", *Nutrition and Health*, 1993, Vol. 9, pp. 165-191

and

"Ear exostoses, Anne, are only found in populations that dive (usu. for shellfish)."

Jan 11, 1999 sci.anthropology.paleo

"We have the ear exostoses (only seen in long-term cold water divers)..."

May 11 1998 sci.anthropology.paleo

"Auditory exostoses, bony swellings of the ear canal, a condition well-known to otolaryngologists, occur exclusively as a direct result of long-term exposure to relatively cold water..."

Jan 10 1999 sci.anthropology.paleo

"The auditory exostoses in Ns prove that those people frequently dived in water colder than 19°C."

Apr 30 1998 sci.anthropology.paleo

These are bony growths in the ear canal, seen in a small number of Neanderthal (apparently 2) and Homo erectus fossils (apparently 1), that Verhaegen says only occur after a great deal of time (he usually says years) either swimming or diving. He calls them "undeniable indications of frequent diving" (sometimes he says swimming) and says that they must have been wading and diving because "Their ear exostoses leave no other choice..." (ellipses in original -- links to these posts are in the references). But ear canal exostoses actually can be due to almost any irritation, most commonly either cold air or cold water. They are not due only to cold water, as he repeatedly claims, and this is widely known and readily available in medical texts (and Verhaegen is a medical doctor). Even online medical sources mention this:

"Exostoses occur when the external canal is repeatedly exposed to cold air or water." "Geriatric hearing loss: Understanding the causes and providing appropriate treatment" by Michelle C. Marcincuk, MD and Peter S. Roland, MD

"Exostoses are bone growths that often develop when the ear canal is repeatedly exposed to cold water or cold air." Yale New Haven Health Library

"The exciting cause of ear exostoses, where the predisposition to these exists, may be anything mechanical or chemical that produces prolonged irritation, with consequent hyperaemia to inflammation of any part of the bony meatus." Ales Hrdlicka quoted in Dry Bones, Chapter 4."

The obvious question here is how can a medical doctor be ignorant of the medical literature on such a well-known ailment about which accurate information is so readily available?

But certainly ear exostoses are associated with -- though not exclusive to -- swimming and diving, and do in fact help show whether a population is regularly engaged in these activities... but this, rather than being a boon for Verhaegen's claims actually helps negate them. If Neanderthals were, as he claims, swimming and diving regularly, you'd expect to see far more specimens with ear exostoses. But we don't -- we see two. This, frankly, sounds a lot like the normal percentages you see in regular, average populations (which range from about 3-10% -- and in some inland groups as high as 20% -- while coastal populations show around 30%). And in modern humans who actually do a lot of swimming and/or diving, the percentage is even higher, in the 40% or more ranges.

So it seems that:

a) Verhaegen is wrong that ear exostoses could only have come about through swimming and diving, and

b) The small number of such exostoses in hominid fossils is evidence against their being in any degree aquatic rather than being evidence for it.

Verhaegen "solves" this problem in classic (and all too typical) AAT/H proponent fashion:

"The presence of AEs proves diving habits, their absence does not."

Verhaegen, May 4 1998 sci.anthropology.paleo

i.e., if the evidence is for you, embrace it; if the evidence turns against you, wave it away -- better yet, do both at once!.

On "*semi-aquatic*" or "*predominately aquatic*" rhinos, "*semi-aquatic*" mountain beavers:

(for instance mountain beavers in Marc Verhaegen and Pierre-François Puech, 2000, "Hominid lifestyle and diet reconsidered: paleo-environmental and comparative data", *Human Evolution* 15: 151-162; rhinos ("3 of the 5 species of rhino are as aquatic as hippos are") online on 1999/05/29, in print (rhinos claimed to be "*predominately aquatic*") in Marc Verhaegen, 1985, in "The aquatic ape theory: evidence and a possible scenario", *Medical Hypotheses* 16:17-32)

As is common for AAT/H proponents, Verhaegen seems to think any animal which sometimes or seasonally wades is correctly classed as semi-aquatic (or even "*predominately aquatic*" as he's said for rhinos). Mountain beavers (a very interesting little mammal of western North America; they're very shy and have very primitive kidneys for a mammal) are not even semi-aquatic; Verhaegen is either confused by the name or thinks that liking -- in some mountain beavers -- damp burrows makes one "*semi-aquatic*". This is not so in any reasonable sense of the term -- and if he was confused by the name it shows he did astonishingly little research on the animals.

The "aquatic rhinos" claim I've dealt with on the ["Rhinos, Pigs, and selective evidence"](#) page.

"hairless" male steller's sea lions, walrus

(for instance Feb 22 1999 ("*Male Steller sealions have neck hairs, but are otherwise furless.*"), Dec 14 2002, Dec 27 2002 in sci.anthropology.paleo)

Verhaegen has been making this claim regularly for years, yet the real facts are not only well known but easily available, for instance in general encyclopedias. And since the advent of the web search it has taken only minutes at most to find these facts, yet Verhaegen still didn't know this one until I pointed it out to him in 2005 (in response to his post which began "I think not, you fool" to my post which accurately said "I think Marc has incorrectly claimed that Steller's sea lions are hairless except for the male's mane.") This speaks of either incredibly poor research or deliberate dishonesty -- which one it is in Verhaegen's case I don't know for sure -- although the latter seems more likely since, although he hasn't yet repeated this false claim in a newsgroup he did repeat it the following month in his own Yahoo group ("*...but mammals that spend some time outside the water in cold regions retain fur, except the very large adult male Steller sealions, walruses and elephant seals.*" Nov 15, 2004). And I know he saw my post because he not only replied to it, but, quite uncharacteristically, admitted he was wrong and thanked me for pointing it out, so his repeating the claim later can't be excused by ignorance.

Actual facts:

Walrus, from <http://www.pinnipeds.org/species/walrus.htm> "Females and younger males have a relatively thick brown fur. Older males have a thinner coat of hair that allows you to see their skin..."

Steller's sea lion, from North Pacific Universities Marine Mammal Research Consortium (four participating institutions: the University of Alaska, the University of British Columbia , the University of Washington, and Oregon State University). http://www.marinemammal.org/steller_sea_lion/steller_sea_lion_fastfacts.php

"Coat: Male Steller sea lions have a thick mane. To protect themselves from the cold temperatures and from jagged rocks, Steller sea lions have thick coarse fur when dry, and smooth, slick fur that lies flat against the skin when wet. Stellers molt for about 4 weeks in the late summer, or early fall.")

From 2003 COSEWIC (Committee on the Status of Endangered Wildlife in Canada) Assessment and Update Status Report on the Steller Sea Lion *Eumetopias jubatus* in Canada

"Pelage of both sexes is comprised of short coarse hairs." pg. 11

From a study on moulting patterns which describes how the pattern of the moult over the body of bull Steller sea lions is similar to that in females. Note that at no time is the body of bulls or females not covered with hairs, either the old hairs or the new ones pushing the old ones out during the moult.

1999 *The Timing of Moulting in Wild and Captive Steller Sea Lions (Eumetopias jubatus)* by Raychelle G. Daniel

"Steller sea lions do not undergo a catastrophic moult, with a sloughing of the entire epidermal layer, similar to elephant seals. Instead, each new hair replaces and pushes out the old hair from the shared follicle." pg. 10

"Bulls appeared to moult in a similar fashion to later moulting adult females" pg. 31

"most-sweating" sea lions:

"nonsense... Don't you know, bk, that the most sweating mammals known are furseals on land" Verhaegen, Dec 25 2003 sci.anthropology.paleo

"The most-thermoregulatorily-sweating mammals besides humans are sea-lions at the shore..." Verhaegen, Jan 17 2004 sci.bio.evolution

Verhaegen has often claimed that the animal, after humans, which uses sweat cooling the most is the sea lion (sometimes he says the fur seal, but when he does this he is apparently confusing this bogus claim of his with the "eccrine sweating seal" bogus claim of his that I describe next):

I go through the process of explaining how wrong this is on my ["Skin, sweat, and glands"](#) page. Perhaps the most damning aspect of this claim of Verhaegen's is that the contrary info, the info that shows it to be wrong, is in the very references he uses to attempt to support his claim. This indicates that either he didn't read the references he used, or that he did and deliberately reported it incorrectly.

On eccrine seal sweating:

For instance *"fur seals are the only non-human mammals which sweat thermoactively through abundant eccrine glands"* in 1991 "Human regulation of body temperature and water balance", in *The Aquatic Ape: Fact or Fiction?* Edited by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds. Souvenir Press: London.

(and Dec 28 2002 ("*Thermoactive eccrine sweating is abundant in sealions & humans on land. I have no examples of this in other mammals.*"),

Dec 30 2002 ("*Abundant eccrine thermoactive sweating is only seen in humans & sealions on land.*") in sci.anthropology.paleo)

He apparently got the bogus "eccrine seal sweating" claim from Morgan (same statement, same reference) and this is something that is easily shown to be untrue, and which I pointed out wasn't true back in November of 1995. (He also sometimes seems to get this confused with his "most sweating" bogus claim and claims that this claim refers to "sea lions" but then further confuses things by parenthetically explaining that by "sea lion" he's referring to the "Pribilof fur seal".) I go through the process of explaining the reality (or rather the lack of it) behind this claim on my ["Skin, sweat, and glands"](#) page. This false claim is not only damning in itself, it's also an example of "incestuous citing", which I describe further below.

One of the odder claims Verhaegen has made is the "snorkel" idea; it has two parts. First he claims that proboscis monkeys use their noses as a snorkel when swimming, which is simply untrue.

"All elephants & proboscis monkeys use the nose as a snorkel to cross rivers or to swim." Verhaegen, May 4 1998 sci.anthropology.paleo

Look, this simply isn't true -- proboscis monkeys hold their heads up out of water far enough so their nose clears the surface, just like other monkeys do, and in fact just as most other mammals do, including humans when they do the dog paddle, which is the swimming stroke that primates -- and most all mammals use other than specialized swimmers -- use. He's also claimed that baby proboscis monkeys float on their backs when swimming with their mothers, when they actually cling -- usually to the mother's back, but sometimes to the mother's front, underwater, during short swims.

Then he claims that Neanderthals looked the way they did because they had sea otter-like habits, floating on their backs cracking clams, while their noses acted like snorkels (he also claimed, at least for some years, that Neanderthals' nostrils were on the tops of the noses instead of the bottom). So ridiculous is this notion that even Verhaegen seemed to feel embarrassed by it when it came up online:

*"*I* didn't come up with "snorkels" here. Some antiAATer did. No doubt, to ridicule AAT instead of discussing it."*

Verhaegen, Dec 5 2001 sci.anthropology.paleo

And indeed one did (Phillip Bigelow on April 1st of 1998) but he did so by accurately quoting none other than Marc Verhaegen's claim, in a 1991 *Medical Hypotheses* article, that *"In a Neanderthal swimming on his back, the large nose with distal nostrils and the protruding midface surrounded by large sinuses functioned as a snorkel."* It seems hypocritical of Verhaegen to decry someone bringing up one of his own statements because the statement might "ridicule AAT".

"Besides, O.Hauser, who discovered the Moustier Neandertal, said the Neand.nostrils were on the top of the nose rather than underneath it."

Verhaegen, Sept 18 1998 sci.anthropology.paleo

And he claimed on several occasions that Hauser (who was famous for being an unreliable amateur fossil finder who, among other archaeological sins, reburied and unearthed the Moustier specimen several times so he could "discover" it for important visitors) saw the soft tissues of the nose of this fossil:

(as Otto Hauser described after the discovery in 1908 of the Moustier fossil: soft tissues can often still be discerned after unearthing, in the seconds or minutes before the soft tissues fall apart).

Verhaegen, Apr 3 and 28 1998 sci.anthropology.paleo

This is nonsense. And his source was a popular book written decades after the fact, not anything by Hauser (which is available, after all). So not using a primary source, using a pop book as a reference for a scholarly article -- these are horribly bad policy for supposedly scholarly work -- but even worse, it's not even what Verhaegen's reference source really said. What Hauser supposedly saw (and since he was a notoriously unreliable excavator and self-promoter we can't be sure he's even saying that honestly) was an imprint in flint flakes that were under the skull. We have only Hauser's word for this, and it seems unwise to trust him given his history -- even more unwise to trust his claim secondhand. So why did Verhaegen do so? He was asked that online, and he replied *"I once read it somewhere, and I believe everything what I read..."*.

Don't do that.

Verhaegen has gradually changed his view from a snorkel-nosed sea otter-like swimming Neanderthal with nostrils on the tops of their noses to just a snorkel-nosed sea otter-like swimming Neanderthal with nostrils on the ends of their noses. I guess that's an improvement (but I'd suggest you *not* follow his lead).

"If you had read me carefully, you should have known that IMO an external nose arises only (& not necessarily) when a mammal starts becoming aquatic..."

Verhaegen, Jun 26 1998 sci.anthropology.paleo

Besides several species of bat (the long-nosed and the leaf-nosed, for example) there's the saiga antelope:

"The most striking feature of a saiga is its large head with a huge mobile nose that hangs over its mouth." (Animal Diversity Web)

And where do they live?

"*Saiga tatarica* inhabit dry steppes and semi deserts." (Animal Diversity Web)

Again, just making things up is not the route to good science.

"Incestuous citing" is a method whereby a "fact" that has no backing can be made to *seem* to have backing by citing a source within a circle of like-minded researchers -- not a source with accurate information, but one who has inaccurate information, sometimes bogus interpretations, sometimes speculation, sometimes simply made up "facts". Incestuous citing can even be done by citing yourself. This is one of the ways that "false facts" can "endure long", as Darwin said, and can be done either as a deliberate deception or completely unintended, as is done many times in urban legends. For instance, the urban legend about "drinking 8 glasses of water a day" was simply repeated, apparently honestly, for years (at least) before a researcher looked into it and found no basis for it -- the fact that so many people said it made it easy to find some source one could cite saying it, but at the root of all those people saying it there was, it turned out, nothing to support the claim.

Possibly the most common sources in AAT/H circles of incestuous cites is Elaine Morgan, because she has written so much, and so many "false facts", on a variety of subjects dealing with the AAT/H. Besides the bogus seal sweat cite I mentioned just above, one common cite, used by Verhaegen as well as others, is Morgan's claim that the proboscis monkey is either commonly, predominantly, or often bipedal on land. There is no evidence for this -- in fact it's contrary to primatologist' observations, but it refuses to die (exactly as Darwin suggested "false facts" would endure) because one can always cite Morgan as saying it, even though she just made it up after seeing a short segment of a nature film. False "facts" like this are common in some politically motivated debates over science and are often called

"zombies" now because they refuse to die (examples abound in issues like DDT use and various anti-global warming claims).

On the subject of diving there's an example brought up by referee 11 below (see referees' remarks).

These are nowhere near the only such incestuous cites within AAT/H circles; in fact such cites seem to be becoming more and more common as the group of AAT/H proponents grows. Let me stress, as I did above, that these few examples are not the only examples available, despite that recent tactic by AAT/H proponents suggesting that the only problems with any proponent's ideas or claims are those I've written down here. I'm simply offering a few examples out of many such possible examples.

Referees remarks to one of Verhaegen's papers

The following remarks are by referees for a paper that Marc Verhaegen co-wrote, which were in an online version of the paper that he offered for download. That version (with the remarks) is no longer online, as far as I've seen, and instead he has a differently titled version of what is essentially the same paper available (the original version without the remarks is still available online). The original was apparently in the poster session of a conference, the "3rd Conference on The Evolution of Language April 3rd - 6th , 2000" as "The Origin of Phonetic Abilities: A Study of the Comparative Data With Reference to the Aquatic Theory" (and the written version with the remarks was apparently intended for the published proceedings of that conference) and the new, barely changed version was published as "Possible Preadaptations to Speech. A Preliminary Comparative Approach" by Marc Verhaegen and Stephen Munro in *Human Evolution* 19: 53–70, 2004.

The changes were minor, such as changing "goldfish" to "small fish" in the following:

OLD: "Humans do not have to chew raw oysters in order to eat them, and can swallow goldfish whole."

NEW: "Humans do not have to chew raw oysters in order to eat them, and are able to swallow small fish whole."

This was apparently in response to one of the referees (referee 6, below) mentioning the "goldfish" remark unfavorably, but unfortunately Verhaegen and Munro didn't change any of the botched references and other problems mentioned (see especially referee 11 below). Why he left these remarks in I don't know, but they are illuminating so we can only thank him for doing so. Note especially the

middle section of the quoted comment by referee 11, which hits on just what is so difficult about countering the AAT/H proposals -- it describes a central problem with virtually every account by virtually every author -- certainly the best known and most influential ones which are by Morgan and Verhaegen:

"A free-ranging, poorly focused paper on "phonetic preadaptations for language"."

...

"All in all, an undisciplined paper with little serious insight to offer."

-- referee 5

"I can confess to knowing so little about the AAT that I am neither for nor against it in principle. I welcomed the opportunity to read this paper, as a way of informing myself about the central issues. However, I found it extremely difficult to extract the basic shape of an argument from this paper, and I fear that any reader coming to the theory for the first time would have the same problem. This could be argued not to be an issue, if the paper offered something of significance to those who already know about the theory, but as far as I can tell, this is not the case."

"To be specific, the paper seems to dance around a set of general assumptions about the AA theory, without ever articulating them. Yet, in the detail that is given, I could not find any clear indication of what the authors were saying that was new. It seems to all be a reiteration of established observations, and though the authors do, no doubt, offer new insights, it simply isn't clear to the reader where they are."

"Through much of the paper, I could not see where the account was going, nor what the relationship was to the general question that needs to underlie all papers in this book, namely, 'how did language originate?'. There are certainly snippets of relevance here, but there is no coherent story at all. If the paper is accepted, then the authors must pay some serious attention to the 'so what?' factor, and explain, at regular intervals, where they are going with the account, why, and how it is relevant to the underlying question."

...

"p.9 para 3 of 'Feeding – mouth and tongue', what is the purpose of the observation that humans can swallow oysters whole, and why on earth are goldfish mentioned (who is swallowing goldfish whole other than a character in 'A Fish Called Wanda'?)"

-- referee 6

"This paper is, loosely speaking, an argument for the AAT of human origins. However, the paper does not attempt a specific argument in favor of the AAT but instead adopts the strategy of presenting a loosely-organized mass of data all of which is intended to have some bearing on the AAT. Thus the paper consists of a collection of "facts", some of questionable veracity, assembled in support of the AAT, mostly drawn from the literature on this subject. Many of the statements that

are made are not supported by references at all, and many (if not most) of the supporting references that are cited are inappropriate, inaccurate or both.

For example, humans are cited on page 3 as being more efficient divers than other primates, but the citations are all to other AAT papers, and only one (an unpublished Swedish PhD thesis) actually contains data relevant to this claim, and in fact concerns only human diving, not that of other primates. Unpublished abstracts by Fitch and Ohala are cited instead of the appropriate and abundant peer-reviewed publications. Other citations are simply inaccurate (the Schlaug et al 1995 [note misspelling of author's name] paper does not show enlargement of "temporal cortex" in response to musical training, but simply raises this possibility in discussion). Deacon's (1997) "Symbolic Species" is cited repeatedly (7 times) as if it were a source of primary data, when it is a popular book. Other of the "facts" presented are simply incorrect."

...

"It would take days to go through the paper and fill in all of the missing references, eliminate the incorrect ones, and check each one of the citations."

...

"I am not usually harsh about reviews. So it may be worth explaining that I am sympathetic to the AAT, think there's something to the hypothesis, and I certainly believe that it deserves air time. This said, I am frustrated by a propensity shown by proponents of this theory to shoehorn data into a form that seems to support the AAT, and to ignore data that goes against it. The current paper is a mess in terms of both the "facts" and the citations."

-- referee 11

There are several aspects of Verhaegen's online arguing style that may sound like *ad hominem* complaints (and which I thought of not pointing out for that reason) but which I think are indicative of his methods over and above the problems I mentioned above.

Online, besides his numerous insults in several languages, he commonly makes comparisons of himself and/or the AAT/H to Wegener (starting with his very first online post, after only a few days online), and he also commonly makes more offhand comparisons of himself and/or the AAT/H to Darwin, Galileo, and Einstein, as well as comparing the AAT/H theory to Copernicus'). This something that virtually all "net kook" or "crackpot" indexes feature as a prominent warning sign (probably the best known of these indexes is ["The Crackpot Index: A simple method for rating potentially revolutionary contributions to physics"](#) by John Baez).

Another suspect tactic is inflating credentials; in Verhaegen's case he's done this by claiming an affiliation with "Studiecentrum Antropologie". It turns out that

"Studiecentrum Antropologie" is not a real institution at all, as Marc explains to a supporter in his Yahoo group:

"In fact, it's "Studiecentrum Antropologie". My Institution is not very large (about 1 room) :-D but if you want to get something published, I thought you better have some Institution. I got this idea after a correspondent on mine (Jos Verhulst) created his "Louis Bolk Centre" (about 2 rooms) for the same reason."

If you read the sci.anthropology.paleo newsgroup, you may see him referred to by his nickname, "macro-man", which he got due to his habit of continually posting one of a few versions of an uninformative mishmash of his theory without regard to whether it's at all apropos to the thread in question. This behavior in others (common among creationists, for instance) has been described, in a lighthearted but nevertheless serious and accurate way, as "*Argumentum ad Assertionem Repetitio ad Nauseam*". Here's an example:

In 1960 Alister Hardy ("Was Man more aquatic in the past?" New Scientist) described how a sea-side lifestyle - wading, swimming, collecting edible shells, turtles, crabs, coconuts, seaweeds etc. - could explain many typically human features that are absent in our nearest relatives the chimps, and that cannot be explained by savanna scenarios: very large brain, greater breathing control, well-developed vocality, very dextrous hands, stone tool use, reduction of climbing skills, reduction of fur, thicker subcutaneous fat tissues, very long legs, more linear body build, high needs of iodine, sodium & poly-unsaturated fatty acids etc.

Hardy was only wrong at the time in thinking this seaside phase happened more than 10 Ma. Early Pleistocene Homo fossils or tools have been found in Israel, Algeria, E.Africa, Georgia, Java. When sea levels dropped during the Ice Ages, H.ergaster-erectus followed the Mediterranean & Indian Ocean coasts. Although most Pleistocene coasts are some 100 m below the present sea level (IOW the fossil & archaeological record often shows the inland Homo populations that entered the continents along the rivers) Homo remains have frequently been found amid shells, corals, barnacles etc., in European, African & Asian coasts, throughout the Pleistocene (eg, Mojokerto, Terra Amata, Table Bay, Eritrea), and even on islands that could only be reached oversea (Flores 0.8 Ma).

The reason Verhaegen is such a source of false "facts" and nonsensical statements is perhaps explained by his incredible admission in response to someone asking

why he believed a particular piece of nonsense: *"I once read it somewhere, and I believe everything what I read, at least until I find evidence of the opposite."* This folks, when it comes to science, is a recipe for disaster.

Update 1 July 2009:

I see that several times in the past year (17 April 2009, 09 Jan 2008, and 20 Sep 2008) Marc Verhaegen made online attempts to squirm out of this bizarre quote of his, once even starting a new thread to attempt to push this rewriting of context.

moore is even too stupid to realise that this was about his savanna

nonsense: i believed it until the facts proved it nonsense

Marc Verhaegen 17 Apr 2009 sci.anthropology.paleo newsgroup

But in fact his statement (Marc Verhaegen 8 January 1999 sci.anthropology.paleo newsgroup in reply to Dan Barnes' post of 8 Jan 1999) was *not* about savannahs, but about Neanderthal nostrils:

(Verhaegen 4 Jan 1999):

>>I had got the impression that at the

>>moment of the discovery, before the soft parts fell apart, the nose etc. had

>>been discernable for a short time (I believe it's less than a few minutes).

(Barnes 8 Jan 1999):

>Where did you get this information from as it is clearly wrong?

Verhaegen (8 Jan 1999):

I once read it somewhere, and I believe everything what I read, at least until I find evidence of the opposite.

Omega 3 fats and the brain

Brain food

Starting a decade or so ago, a couple of researchers in the field of nutrition got involved with the AAT/H. This is an interesting example of the usefulness, or in this

case the lack of usefulness, of credentials. As far as I know, Michael Crawford and Stephen Cunnane have excellent credentials in the field of nutrition; at the very least they've been connected to good universities and gotten what certainly sound like prestigious positions in their field. But when they walked from their familiar field into the field of human evolution, they stumbled badly.

This is related to the fatty acids DHA (docosahexaenoic acid) and LNA (linolenic acid), often referred to as Omega-3 fatty acids. They are indeed needed for brain growth, but there are a couple of major fallacies promoted in saying that this requirement is evidence for an aquatic past. In regard to these fatty acids, Crawford and Cunnane are trying to do something good, but are apparently seriously ignorant of evolutionary theory. The good thing they want to do is push the addition of DHA into foods, such as milk and eggs, much as we have long added vitamin D to milk. This is useful because many people don't eat the kind of diet that would give them a proper dose of DHA or LNA, as our ancestors' diets would. The push for additional DHA in modern diets is overall a good one, in my opinion, but in trying to tie this laudable goal in with a dubious theory, they do themselves no favors.

The argument, in short, is as follows (and contains a classic logical flaw):

DHA is a requirement for normal human brain development

DHA is abundant in fish

hominids needed to eat fish for normal brain development

The first two statements are true -- definitely true, absolutely no question about it. But the third statement is false. The reason the third statement can be false even though the first two statements are true is that the argument is missing information (Fallacy of Exclusion) which makes it seem as though the only place where enough DHA is present is at the shore. And in fact Crawford at least has explicitly (and quite falsely) claimed that "the only place you can do that is at the seashore". This claim just isn't true.

First, we don't really need loads of DHA, and humans (except for infants) can synthesize the necessary DHA from LNA, which is available in vegetable oils as well as meat sources such as wild game (infants get it from breast milk). The Mayo Clinic, which has always had a strong nutritional slant to their medical practice, says "One tablespoon of vegetable oil easily meets your daily requirements." And people who've calculated the amounts of energy and DHA and/or LNA fatty acids needed to develop normal brains note it's available even in savanna conditions. They also note that these non-seashore environs offer foods which are richer in other required substances, so the notion that the seashore environment was so much easier to collect food at is only true if you concentrate only on a small portion

of the dietary needs. Naturally, you shouldn't do that, but AAT/H proponents who use the Crawford and Cunnane argument do just that, and so instead of looking at dietary factors in terms of whether there's enough of all required parts of the diet in a given environment, they look at where there's lots of only one part of the diet.

Data gathered shows, for instance, that the required DHA is available in savanna and other terrestrial environments as well as that the purportedly required shore-based diet is far lower in energy (also required of course) than the terrestrial diet (Loren Cordain, Janette Brand Miller, S Boyd Eaton, and Neil Mann, *American Journal of Clinical Nutrition*, Vol. 72, No. 6, 1585-1586, December 2000). The problem for a shore or fish-based diet is made worse by the fact that the especially rich sources of DHA in fish are cold water marine fish which are certainly not the type of fish available -- by any reasonably conceivable scenario -- to our ancestors at the time we see increased encephalization (1-2 mya). They were simply not within a thousand miles of them. The fish that were available were -- so to speak -- a different kettle of fish.

They have DHA, certainly, but actual measurements show they are a no better source than such terrestrial sources as brains, which were highly likely to have been scavenged by hominids (along with marrow) by the time we see the first great brain expansion in our ancestors. They, and other shoreside foods, are also very considerably lower in energy than savanna and other terrestrial foods, making the claim that the shore was a much easier place to get the required diet even more unlikely.

Dietary determinism

Another serious problem here is the dietary determinism inherent in this idea. I've described before how environmental determinism, as practiced in the AAT/H, makes the foolish assumption that environment trumps all when it comes to evolution. Dietary determinism does the same, only with diet as the primary selective force. The theory, as promoted, essentially makes the claim that a DHA-rich food source would somehow give a kickstart to our brain growth, and that just doesn't square with the way nutrition works. The fallacy in the idea of the supposed need for an aquatic diet to get larger brains is the same fallacy that people use when they take megadoses of vitamins -- too little isn't good, enough is good, so lots more is better. Being trained in the science of nutrition, both Crawford and Cunnane should know better, even if they don't understand evolutionary forces very well, but they seem not to understand this, since they do make the mistake.

In dietary issues, you essentially always see the same thing: more is not necessarily better. Often it's far worse -- many dietary necessities, such as vitamin A (and phosphorus for that matter) are toxic in too large a dose. Generally you find

that deficiencies hinder growth or health, but larger than needed amounts (within a range which varies between individuals) don't create more growth, development, or health, and they often have the opposite effect. This is a common problem, especially perhaps in North America, where we tend to often think that if a little is good, more and more is always better. With most things there's a level that's needed and more does nothing, or nothing good. This is a problem with dietary supplements which always needs to be kept in mind, and is why most dietitians suggest a varied and balanced diet as the best way to obtain good nutrition.

One of their critics sums up the core problem with the Crawford and Cunnane idea: "In this Lamarckian scenario, the quiescent brain appears to be waiting patiently for humans to discover aquatic foods and then, eureka, the brain is free to enlarge and modern humans result. Not only are the selective pressures involved in this scenario unspecified, no information is provided as to how these large-brained humans were then able to provide DHA and other brain-specific nutrients for themselves or their developing offspring once they moved away from lacustrine or shore-based environments." Katharine Milton, pg. 1587 *American Journal of Clinical Nutrition*, Vol. 72, No. 6, 1586-1588, December 2000

In other words, if you want super-smart monkeys, you're not going to get them by stuffing fish down their throats.

An obvious null hypothesis

One handy tool to use in science is the *null hypothesis*, which consists of -- to be simplistic -- taking an idea, stating pretty much it as its "opposite", and see if you can make as good a case for the opposite idea. You shouldn't be able to, not if the original idea was accurate. If you can, then you've tested the original idea and found it wanting as an explanation -- but it doesn't mean your alternate, "opposite", explanation is accurate. In fact, if both opposing explanations work equally well, neither one is good on its own -- you may be able to combine them into a new, accurate explanation, or you may need to start over. But what you have done is to test the first idea.

In the case of the idea of seashore DHA being needed for brain growth, there are a couple obvious ways to test the idea with a null hypothesis.

One is to ask, were there (before modern supplements say, that would be before 100 or so years ago) people who developed normal brains in the absence of seashore diets? Yes -- rather obviously, there were. So even without any other testing or knowledge we are able to test the idea and finding it lacking in explanatory power.

(You can apply this question to fish-eating or other shoresides as well; all you have to do is realize there are people who didn't/don't eat sea, lake, or riverside foods who nevertheless had/have normal brains -- that's it, you've shown the "we needed shoreside foods for brain growth" idea to be false.)

A second null hypothesis test is suggested by the comparisons Crawford made in his statements on David Attenborough's spring 2005 BBC Radio 4 show, *The Scars of Evolution*. I mentioned it on my page which critiques that program, and have other info there on brains and diet, and how you can compare brain sizes in animals of different sizes (using their *EQ* or "encephalization quotient"). If dietary determinism were a valid way of looking at this problem (as Crawford suggests -- and Attenborough suggests even more explicitly -- on that show) you need to wonder why all creatures which eat fish don't have huge brains. Diving ducks, sea gulls, herons, otters, seals -- why aren't they at least dolphin smart? Mind you, they shouldn't necessarily be, not if you realize that large brains are not always an advantage, as all sensible evolutionary theorists do. But if you're not sensible, use dietary determinism, and imagine that diet trumps all, what about all those not overly bright fish-eating species? Why are the smartest of them, outside of only a few dolphin species, only as large-brained as monkeys and apes which eat mostly fruit and nuts?

It doesn't hurt to do more testing than these two null hypotheses -- for instance the nutritional data people have rounded up concerning the relative value of seashore and other diets vis a vis our evolution is terrific and ultimately useful, or at the very least interesting. But this data isn't actually needed to counter the seashore diet and brain growth idea. The real problem here is that this particular idea is so easy to counter, using one or both of the above null hypotheses, that it shouldn't have seen the light of day, at least not in anything like the form it did. It should've been thought through and had things like a null hypothesis applied to it by the original authors of the idea even before they wrote their first drafts of their first papers on it. The problems with it are *that* basic.

Algis Kuliukas

His "mild" version of the AAT/H

Algis Kuliukas is a former post-graduate student (at present in Western Australia) who has become one of the major proponents of the AAT/H. He got a master's degree in anthropology at University College London in 2001 and went for a PhD at University of Western Australia from 2003 until late 2008. He did not complete his PhD.

He has been trying to make a claim for a "milder" version of the AAT/H for a while now. This is an extension of the internally inconsistent manner in which the idea has been presented by virtually all of its proponent's for many years, what I've dubbed its "ZING!ability". I've described this, and the issue of Kuliukas's attempt at defining the AAT/H, on my "General Problems" page.

And by the way, when Kuliukas talks about our ancestors wading, he's talking about *wading sideways*. He has also suggested that "waddling, a kind of ice-skating mode, was intermediate between the two" ("the two" being forward and sideways). When confronted with the fact that he claims this, and peoples' reactions to it (which do tend toward incredulity) he's said that he should've used the words "lateral motion" instead of "sideways wading"; in other words the problem lies not in what he said but that the words he used were too easy to visualise, and a more obscure statement would be better. Of course "lateral motion" is not that difficult to comprehend; I think most people could see that he was still talking about wading sideways. He expects others to "agree that the evidence that Lucy might have moved in this way is quite staggering" which would come as a surprise to those who've studied Lucy and other australopithecines

He also exhibits the by now standard AAT/H coyness about what animals he's talking about when it comes to characteristics, but as always the characteristics are those of seals, whales, and sirenia -- yet we somehow got these features via convergent evolution by wading, and only some occasional diving and swimming (and the wading is *sideways*, remember).

Like all the major AAT/H proponents, he engages in special pleading. Take a look at this claim from the sci.anthropology.paleo newsgroup as an example:

"Stop waffling and let's get back to the point: Maximally efficient human bipedalism, based on the inverted pendulum (straight legged) gait simply does not work in habitats that are not flat, firm and relatively vegetation free."

--Algis Kuliukas Jun 3, 2004 sci.anthropology.paleo

Now that's a true statement -- absolutely incontestable -- but one which somehow leaves out something that is, just by the greatest of coincidences, damaging to his argument. One of the places where "maximally efficient human bipedalism" "simply does not work" is an aquatic environment, whether shallow or deep.

There's actually a bigger -- and perhaps more subtle -- problem with the statement: this is the use of the qualifier "maximally efficient". Most of the time bipedalism will not be, and should not be expected to be, "maximally efficient".

Carrying or feeding, for instance -- two activities where we commonly see non-human primates use bipedalism -- would render the bipedalism less than "maximally efficient". So would walking on soft ground, uneven ground, wet ground, walking up or down even the slightest of slopes, walking through vegetation, or around rocks, roots, and snags -- and a moment's reflection should cause you to realize that all those conditions also occur in water, with the added problem that they often aren't as easy to spot and therefore compensate for.

In this way he takes an incontestably true statement and, by leaving out an apropos but inconvenient bit of information, turns it into something that needs ironic quote marks -- a "fact". And he uses this abbreviated "fact" to claim that "our bipedal locomotion is not suited for woodland, dense bush or tall grassland" (Algis Kuliukas 06-21-04 sci.anthropology.paleo). Let me ask you, have you ever walked through a woodland, through some brush, through some tall grass? Did *you* find it as enormously difficult as Kuliukas suggests? I've done it and it just wasn't so difficult; I can't be *that* unusual.

This is just one example of what has become the classic AAT/H proponent's habit of ignoring contrary info; another example from Kuliukas is what he did when he found the data on hair and swimming speeds on my site (mentioned on this site's [Hair and "Hairlessness"](#) page). In fact, this habit is so ingrained he seems to be unable to comprehend why on earth I would mention data that doesn't bolster "my" case. I found conflicting data, and therefore wrote up and posted the data both for and against the idea that reducing body hair would improve swimming speed. I did this because that's the way you do science -- it's the only honest and sensible way to do it -- but Kuliukas assumes that I did so "by mistake" (Apr 26 2004 sci.anthropology.paleo). That he would assume so tells us a lot, I'm afraid, about his own attitudes toward data -- it's projection on his part.

And he has a critique of my site online, which can be summed up as underwhelming -- for instance, criticism of my URL (the web address www.aquaticape.org) figures prominently. He also seems to be annoyed that when I described the dishonest leaving out of contrary data about hair and swimming, I didn't mention his name -- I thought I was being kind; he apparently considered that deceitful. Now I've mentioned his name in connection with that episode, but somehow I still don't think he'll be happy -- some people are just hard to please.

His master's thesis

In 2001 Algis Kuliukas, as partial requirement for his master's in anthropology at University College London, did a thesis on wading and human evolution, called "Bipedal Wading in Hominoidea past and present". The second part deals with his obsession with humans wading by shuffling sideways, and whether this awkward,

slow gait creates less drag than wading by walking normally, which seems unlikely to ever get any traction as a reasonable idea; the first part consists of observations of bonobos and does get mentioned, mostly by him, frequently... albeit in a conveniently expurgated manner.

You may see Kuliukas mention that apes are bipedal 92% of the time they are in water; he frequently says this or some variation of it, and it figures prominently on his web site. As is often the case with AAT/H proponents' claims, it's what they don't say that's key.

On the 92% figure, what often isn't seen, or isn't reported, is that this was 92% of 37 seconds total for 9 occasions over 3 days. This is a problem for several reasons, one of which is that he doesn't mention as prominently that those same bonobos were bipedal on land more than 4 times as much. Another of course is that 37 seconds for nine occasions isn't much wading at any one time, yet he obviously considers this to be an example of a major selection pressure. Now it's not that 37 seconds itself isn't interesting and not worthy of reporting -- it's just that Kuliukas has made rather too much of it, and also ignored several important questions he should've asked himself before he even got to the point of writing it up.

When I did my first write-ups (in several online forums and newsgroups) of Kuliukas's thesis I was doing a quick read of his paper and inadvertently gave him credit, perhaps, for doing a bit more work than he actually did. His observations were done over 3 days, and at first I thought he did an actual day's worth of observations each time, but actually his observations totaled only 5 hours over 3 days. Even that may be giving him more than his due -- he mentions the 5 hours, but then also says that the observations consisted of following the actions of 5 individuals for a half hour each and that "one hour's worth of observations were recorded of isolated, real or anticipated instances of contextual wading behaviour". Whether that hour's worth were from the 5 half hour periods or some other periods isn't clear, and I don't know where the 5 hour total came from.

It's interesting how he chooses to present his conclusions (this is indicative of how AAT/H conclusions are generally put forward, by the way). He points out that these bonobos were bipedal on land 1.16% of the time and bipedal in water 92% of the time. That certainly sounds impressive -- a lot more impressive than pointing out that during his observations they were bipedal on land over 4 times as often as they were in the water (in his observations, 161 secs vs. 37 secs -- not a lot in either case). In fact, they were bipedal on land more than 3 times the total amount of time they spent in water (1.16% of the time versus 0.37%). He apparently doesn't consider either of those other points especially significant, and certainly doesn't present either as a bar graph, as he does (twice) for his own preferred presentation.

You may notice that unlike Kuliukas, I tend to mention both the percentages *and* the actual time while he doesn't seem to very often. It's best to mention both, just as it's apropos to mention how much time they spent bipedally on land. Using only the statistic, as Kuliukas virtually always does, seems (and let's hope this isn't so) to be a way of making it sound a whole lot more impressive than it in fact is. It's always a red flag when someone uses a statistic -- a percentage or ratio, for instance (most commonly, in my experience) -- and doesn't mention the actual numbers, and statistics drawn from such a limited sample are a dubious method at best. If someone is claiming that water is ever so much more important than land when it comes to the evolution of bipedality, but doesn't mention that the apes he watched were bipedal on land far more than they were in water, that also raises one of those red flags.

Perhaps the most critical failing in his thesis is that when he looks at the amount of time in water that his subjects were bipedal he gets a number that's far greater than any other study; several times larger than the next largest percentage (which was about 24%; studies of other bonobo groups have shown less than that -- and some bonobo groups apparently always wade quadrupedally). This cries out for examination as to why, but he doesn't do that. When you do a study where one of your measures is way off the graph, it doesn't necessarily mean that the measurement is bogus, but you have to ask why -- why is it so far out of line with everything else. There are several possibilities I can think of but these were apparently never examined; Kuliukas didn't seem to ask himself this obvious question. For instance, is it because the bonobos were getting food? we know that food-getting is a major factor in bipedality; in fact Kuliukas is aware of this, or should be (in fact in one case he mentions the bonobo in question was bipedal on land just before entering the water -- he was begging for food). Is it due to some factor around the way the moat they waded in was constructed -- the slope of the sides or how slippery it was or anything like that? Was it something else? we don't know for sure and Kuliukas apparently just didn't bother looking at that even though that is a question that should immediately spring forth when you measure something and get a number that's so wildly out of place compared to other studies.

Note: Having seen some video of the bonobos and zoo moat in question, it's clear that I was right about at least one of my conjectures. The moat *is* fairly steep-sided, requiring the bonobos to step from the bank into water which is about knee deep, and of course they are also quickly (usually within a second or so) grabbing their target and quickly stepping

out. This makes it quite difficult to do anything but enter it bipedally, although in one of the nine total cases it was done quadrupedally.

The same is true of the number gotten for bipedality on land in Kuliukas's study -- is it similar to what you'd expect in a wild population or is it under the value you might expect? For instance, Hunt's study shows a very large part of bipedality used in feeding, but in captivity the feeding is far different than in the wild, so we might well expect there to be far less bipedality used in feeding in captivity -- this would mean the amount of land-based bipedality could well be greatly underrepresented in a study like Kuliukas's. This too he seems to have left unexamined.

And of course Kuliukas doesn't seem to be getting the distinction between what he did with percentages and what Hunt did -- he just sees percentages mentioned in both cases and assumes they're the same (much as we see when AAT/H proponents see water mentioned in the context of human evolution and automatically assume it's evidence for the AAT/H position). Hunt used percentages to show what activities contributed to bipedality; Kuliukas used what medium was used, without regard to activity. In itself this isn't necessarily a bad move on Kuliukas's part (although he ignores the fact that his bonobos were engaging in food-getting and carrying behavior known to contribute to bipedalism), but the problems arise when he doesn't examine why his numbers for bipedality in water were so many times higher than any other study and when he doesn't see that even in a setting where bipedalism on land may have been less than normal and bipedalism may have been more than normal (neither is examined) -- even in that setting bipedalism on land was used 3 times more than in water. So in terms of what activities might have contributed more to bipedalism Kuliukas's own study shows a 75%/25% split in favor of land -- and for the reasons mentioned above this would seem to be biased toward the water end of the scale.

He also doesn't seem to see any effect on the fact that all the times these bonobos entered the water they were retrieving food that had been thrown into the water (by visitors; he wasn't allowed to and didn't do it) and so of course they needed use of their hands to pick it up. That would of course encourage the choice of bipedal posture (as it often does on land when food is otherwise out of reach).

This is typical of AAT/H research, in my experience; selective reporting of facts, ignoring unwanted data (and sometimes just making things up; this last, thankfully, is something I don't see in Kuliukas's thesis). Frankly, I think he took a minimum of observation and stretched it far beyond where it could reasonably be taken, using percentages to make it sound more significant than it was. I suspect he might better have spent his time in the library digging out the specifics from as many

papers as possible. He also had a long section in there about some tests he did with people wading in a pool to test water resistance, in which he comes up with the idea that wading sideways might have been a primary method used by early hominids, and really, that strikes me as a touch odd.

Also, he's going on about the number of "data points" in his study compared to Hunt's much longer study -- data points in observations of a subject are the periods of time one has looked at one's subjects and there is always some degree of arbitrariness to what constitutes a "data point", but Kuliukas stretches this well past the bursting point. He's is right about the usefulness of using video, when possible, to record observations of this type, but of course this is much easier in a zoo setting than in the wild. Not only are wild observations harder on the equipment (although I expect video equipment and tape is far better at handling this than it used to be) but the sight lines in a zoo setting are generally far less obstructed than in the wild, and since you have them in a confined space, you don't have to worry about moving around much with that equipment. There are also other problems with using video; one big problem with a video record is that you tend to miss more of the overall goings on because you are focused on a small portion of the overall view. Any photographer is aware of how photos and video are a more restrictive view than a set of eyeballs gives you; ironically the eyeball, because it's attached to a massive data-shifting apparatus -- the brain -- can focus in tighter on a subsection of what it views. The camera records what it sees -- all of it -- so it's good that way, but since what it can see is typically far more restricted, you have both good and bad points in using a camera for observations. But overall, especially for observations in a zoo setting, video is a good tool.

But Kuliukas's notion that you get so many more "data points" is at the least disingenuous. The way he does it he concludes that he has 14,400 "lines of data" which sounds pretty good compared to the two studies he compares his with, because they have "21,030 data items" and "30,072 lines of data" respectively. This makes it sound like he's done about half the observing of the second study and two thirds of the other one -- but look at the numbers: the Hunt study with the "21,030 data items" was 701 hours of observation compared to Kuliukas's 5 hours -- 140 times as much, which is about 100 times more than Kuliukas's "lines of data" comparison implies (or about 50 times in the case of the Videan and McGrew study -- 250 hours of observation vs. 5 hours). Even if you use lower "actual number of data items recorded in the database was 1,319" (after skipping "long periods of inactivity") you still have a number that's wildly misleading -- the real difference is about 12 times more than Kuliukas's "lines of data" comparison implies (or about 11 times in the case of the Videan and McGrew study). Is this a deliberate attempt to inflate the importance of his study, or simply an inadvertent misuse of statistics? I don't know and don't care, because it really doesn't matter *why* it's wildly

misleading. Whether deliberately dishonest or simply foolish, it's just wildly misleading and this helps demonstrate the thinking (or lack of it) behind the research.

He seems to be considering each second of his recording a "data point" compared to each 2 minute sampling period in Hunt's study (and each 30 second sampling period in the Videan and McGrew study). This is silly, and ultimately very arbitrary -- why not consider each frame of video a "data point"? just as valid (which is to say "not valid") as using each second, and you get 450,000 "data points" in a 5-hour study (using PAL or SECAM; 539,460 "data points" in NTSC, which makes North American and Japanese research that much better, doesn't it :). So using a video record is often good but entails some trade-offs; but the bottom line vis a vis "data points" is that Kuliukas's claim to have so many more is based on a rather arbitrary and meaningless measure. I'm sure he will disagree with this.

Note: Algis has in fact disagreed with this criticism, or rather, he has constructed a strawman, a false claim about what I said, and knocked *that* down. He says "Moore criticises my claim that using a video recorder to record behaviour has a number of advantages...". As you can see in the preceding section, I actually said "He's is right about the usefulness of using video". I did point out that it has some disadvantages as well as pointing out what they were, then said "But overall, especially for observations in a zoo setting, video is a good tool." I also pointed out, as you can see above, that the bigger problem was his claiming a great many "data points" comparable to studies which were 50-100 times longer, and why that is a disingenuous claim. He either ignored or decided to not respond to that criticism.

Another interesting thing, Kuliukas has later asked (apparently after reaction to his thesis online) one of the zoo workers whether his observations were typical of the bonobos at that wildlife park, and the answer was yes. Interesting that those particular bonobos wade so seldom and for such short periods -- if they do indeed generally just duck in and out for a fraction of a second or a few seconds at most, as Kuliukas reported for his observation period. Because that's the other part; those particular bonobos seem to be rather unusual, as they seem to not wade at all if they can help it, and they wade so very little, unlike virtually all wild bonobos. Does that affect Kuliukas's conclusions from his observations? Not according to him, apparently, although it should, since they seem to be acting quite differently from wild bonobos -- to be expected in one way or another, of course. But to use

observations from that short observation period of apparently unusually acting bonobos as if they were more representative of ape behavior than longer observations of wild bonobos is not a sensible method.

So here's the bottom line regarding Kuliukas's thesis: he took a total of 5 hours of observation over a period of 3 days. Notice that he virtually always uses percentages when he mentions this study, perhaps because it sounds better than saying that these apes spent only a total of 37 seconds in water -- mostly on one day -- and that they were bipedal on dry land over 4 times as long as they were in the water, unless you go with percentages and try not to mention the actual times. Since in his study the bonobos spent less than 1% of their time in the water (less total time in the water than they spent bipedal on land, by the way) this does tend to skew things. Yet Kuliukas wants this study to essentially replace much longer observations which give different results, as well as pretending that this is applicable to all bonobos when we know that they vary in behavior from place to place, much as chimps do in many of their habits.

A further note: in thinking about the relatively low amount of bipedalism Kuliukas saw in his study, I found a publication by another person who'd studied the bonobos in that same park a few years before, and the differences between that study and Kuliukas's were fascinating. Duchêne (1997) took a different tack, and I'd say a far more sensible tack, in looking at bipedalism. Whereas Kuliukas compared the time spent bipedal to total time, Duchêne compared the time spent bipedal to the time spent quadrupedal. This seems to me likely more apropos (and in Kuliukas's case it would still give him the big number he so obviously wants for bipedalism in water, although it would still leave all those unanswered questions he should've thought of and answered). It would, however, give a far more representative number for the bipedalism on land for those bonobos, because most waking time -- the vast majority of time -- on land for these bonobos was sitting around rather than any sort of locomotion ("65% of the time" in Kuliukas's case), while apparently *no* non-locomotor time was spent in water. This wouldn't give Kuliukas the extremely low number he also so obviously wishes to have for bipedalism on land, but it would still be low in his study and would be far more honest.

By the way, in light of what I mentioned above about apes so often using bipedalism in regard to food, Duchêne found that the percentage of bipedalism (compared to quadrupedalism) jumped from "3.9% for spontaneous bouts to 18.9% when abundant food is supplied" -- 4.8 times more because of food. This brings to mind Hunt's finding that "80% of chimpanzee bipedalism occurred most often during feeding, with no other context making up more than 4%" and that "Bipedalism was observed most commonly among chimpanzees when they fed on the small fruits of diminutive, open-forest trees". This also fits with the results of

Videan and McGrew's experiments which showed carrying food as a big incentive for bipedalism in both chimpanzees and bonobos. Remember that *all* of Kuliukas's observations of bipedalism in water were connected with getting food.

We saw at the start of this page an example of how Kuliukas started a true statement and turned it into a false "fact" by leaving out an some apropos but "inconvenient" bits of information. Similarly, he's taken his observations of a true but apparently highly unusual pattern of behavior on the part of 10 captive bonobos and -- by leaving out many inconvenient but apropos pieces of data, turned it into another false "fact". He's then used this false "fact" as if it were true:

*"Since 1994 a great deal of film footage and observational data from the field has shown that the one place apes are pretty much *always* bipedal is in water. This is blindingly obvious to anyone with an ounce of common sense."*

Algis Kuliukas May 2 2003 sci.anthropology.paleo

"Extant apes are pretty much 100% bipedal in water..."

Algis Kuliukas May 13 2003 sci.anthropology.paleo

He's also used the percentages in his thesis study as applicable to all apes, despite the fact that his percentages are so off the charts compared to other, longer studies. He's done this many times; here are a few online examples:

"I'd estimate that the amount of that wading time that was bipedal would have been pretty much as it is in extant apes today - i.e. over 90%."

Algis Kuliukas 9 Nov 2001 sci.anthropology.paleo

"Extant apes wade bipedally - they don't walk in the mid-day sun that way, they don't go long distances that way. They do carry things, feed in branches, perform threat displays and a few other activities that way but only about 2-3% of the time. In water it's over 90%."

Algis Kuliukas 3 Oct 2002 sci.anthropology.paleo

"Confused? Extant apes are 2% bipedal on land, over 90% bipedal in water."

Algis Kuliukas 21 Feb 2003 sci.anthropology.paleo

And despite any later waffling and qualification, these false "facts" will out there to trap unwary and uninformed people whose only "crime" is wanting to learn about human evolution, who will learn and repeat such misinformation entirely innocently. What's "blindingly obvious to anyone with an ounce of common sense" is, sadly, that the biggest legacy of the AAT/H and its proponents, as far as I've seen: a long litany of false "facts", like Kuliukas's statements above, that will ensnare the innocent and no doubt, as Darwin warned, "endure long".

Creating another false "fact"

I've mentioned on my [Hair and "Hairlessness"](#) page how Kuliukas found the data on hair and swimming speeds and since has consistently reported only that part of the evidence I found which supported his case, not that which undercut that support. He also went to a study (by Sharp and Costill) I referenced and selectively reported data from that, turning the possible 3-4% gain reported in that paper (they said body hair removal "may contribute to the 3-4% improvements in performance typically observed at championship meets") into a 12-13% gain, and he's used this false "fact" repeatedly since, relying on others not to notice. Here's how he did this.

Sharp and Costill did several tests of swimmers and hair removal; one was a push-off and glide which demonstrated reduction in drag after shaving all body hair. Mind you, this body hair swimmers feel the need to shave off is the same body hair AAT/H proponents say is the way it is as a result of selection for reduction of drag in water; one wonders how selection of that sort left us with exactly what we don't want in water, but that's another story. In the push-off and glide tests Sharp and Costill fitted their test subjects with a belt and string attached to a low resistance reel and running over a DC generator so they could record the subjects' velocity and the velocity decay -- the amount they slowed down when gliding for 4-6 seconds. It's a pretty neat little test setup.

They tested two groups, a control group who did the push-off and glide on two different days without shaving their body hair between tests, and an experimental group who did push-off and glide on two different days but did shave their body hair between tests. Sharp and Costill listed the slowdown in speeds at intervals, listing 2.0m/s (4.47mph); 1.5m/s (3.36mph); 1.0 m/s (2.24mph); and 0.5m/s (1.12mph). The test subjects showed the greatest difference at the start of their glide when they were at their highest speeds. The reason is basic physics: drag is greatest, and therefore drag reduction most effective, the faster the speed the object is moving. The experimental group slowed less after shaving, demonstrating about 2-3% reduction in drag for those who had shaved their body hair. But that's only if you do it honestly and look at all the data rather than pick out some bit you happen to like. What Kuliukas did was to pick out only one little piece of data at one

of the highest speeds, at which point the reduction was 12.869%, and pretend it's very meaningful on its own. There are major problems with his approach:

1. He pointedly ignored the overall amount of drag reduction, which was considerably smaller.
2. He ignores that this reduction is only for a few seconds as the test subjects slow down.
3. The push-off phase in competitive swimming is far faster than any swimming stroke, which is why the short course records are faster than the long course records for the same distance. The short course pool they use is 25 meters long and the long course is 50 meters, so any swimming event in the short course pool has twice as many turns and push-offs than the same distance swum in a long course pool. The difference is not insignificant; for instance the present (July 2009) world record for the men's 100 meters freestyle is over 2 seconds faster in the short course pool, with its three turns/push-off instead of one.
4. The physics angle: drag is greatest, and therefore drag reduction most effective, the faster the object is moving.
5. The higher speeds where you see this larger reduction in drag are far faster than what our ancestors were likely to be capable of. In fact the 1.5 meters per second speed he likes is just slightly slower than the men's 100 meter freestyle *world record* from 1905; it wasn't broken in women's swimming until 1931. It's still far faster than most people can swim, and of course is far faster than the average swimming speed.
6. This decrease in drag was while in the glide position, which is prone, arms extended, and otherwise motionless. In other words it's not a position which can possibly be used for swimming. Any swimming motions upset the hydrodynamics of this position; in fact much of competitive swimming training is training athletes to minimize extra movement since that extra movement creates additional drag.

So there are a number of reasons you shouldn't take one tiny piece of data out of a study instead of using all the data. Simple honesty is one. Accuracy is another. Taking that one piece is dishonest, but more importantly, it's not accurate.

With this technique, not unlike quote-mining, Algis has created a false "fact" out of a piece of data taken completely out of context. And he's been spreading it around;

at least 7 times in newsgroup postings, more times in at least one online forum. As with his "100% bipedal in water" and his reporting of only some of the hydrodynamics data I found, his false 12-13% claim will be out there to trap unwary and uninformed people whose only "crime" is wanting to learn about human evolution, who will learn and repeat such misinformation entirely innocently.

Vagueness and ZING!ability

I've mentioned before that after Hardy it's been rare that any AAT/H proponent gives their idea of any specific amounts of water use they're claiming for our hominid ancestors. And I've already mentioned (in the "Fallacies of Ambiguity" section on the [General Problems with the AAT/H](#) page) Algis Kuliukas' incredibly, uselessly, vague "definition" of how much water use the AAT/H is talking about, where he only says it was "more" than for our ape relatives. But thanks to some questioners in newsgroups and forums, he's come up with a more exact number. Did I say number? singular? I meant several of them, at various times, and so rather than clarify things it just makes it even murkier. But this does demonstrate one of the techniques commonly used in arguing for the AAT/H, vagueness and shifting, often contradictory claims. For instance, he first offered this:

*"The point is wading *is* very rare in extant apes. But then extant apes are not bipedal, they're quadrupedal. Except in water, when they're pretty much *always* bipedal. Therefore (are you still following) if ancestors of our's went into water *more* (that is more than 0.21% but probably not more than 10-20% of the time) then this is very likely to have been enough to push them over the rubicon where they were likely to be bipedal on the land as well.*

"Please tell me, if this is not clear, how can I possibly make it any clearer?"

Algis Kuliukas 24 Jun 2004 sci.anthropology.paleo

Not super clear -- that's a big range -- and of course this smallish amount of water use doesn't make sense since the various versions of the AAT/H, including the so-called "mild" version Kuliukas advocates, are claiming we evolved the features of mammals which are fully aquatic and have been for tens of millions of years, but at least it *is* a lot more exact than "more then". But by 2009 this had changed radically:

"Well, if you remember I'm suggesting that even very slight selection from moving through water can account for all those differences but that we are/were pretty much 100% (to the nearest integer) terrestrial."

Algis Kuliukas 18 Aug 2009 Richard Dawkins Forum

"Well actually I think I do define just how aquatic we are... not very. I even tried to put a percentage on it (0.4%). So much for that objection."

Algis Kuliukas 23 Aug 2009 Richard Dawkins Forum

Now we've apparently gone from talking about Alister Hardy's hominid, aquatic for half the day for 10-20 million years, to a "pretty much 100% terrestrial" hominid -- which somehow has a tiny amount of contact with water, a fraction of one percent, causing them to evolve the features of whales, sirenians, and seals. Concerning this fraction of one percent Algis says "this explains all the major phenotypic differences between us and them" (us and them being "humans and chimps/gorillas"). How is that possible?

It isn't. It doesn't make the least bit of sense. The so-called "mild" version is the opposite of mild, it's actually far more radical than Hardy's, which didn't mesh with the facts but at least had a time period that might account for those features. And the shifting claims allow the proponent to accuse anyone of misrepresenting what the proponent says. In this way the vagueness and differing numbers act not as a bug, but a feature; you point to one number he's used, he points to one of the times he's used another number, or to the vague no number definition, a "definition" so vague it doesn't define. With this scattershot method, with several contrary claims made by the proponent, no matter how you describe the proponent's argument -- *even if you quote his own words* -- you are always wrong, because he can always point to a different, contrary, claim the proponent has made.

But Algis isn't through yet; one of the oddest facts he's used as if it were evidence for his idea is that Jane Goodall has reported that chimps in her study area often use bipedalism when the ground is wet. Not wading, not crossing a stream, just damp from rain. Here's a typical quote:

I should remind you that in Jane Goodall's landmark survey of chimp behaviour she ranked moving over wet ground (after or during rain) second in her big three factors to compell bipedalism. (Van Lawick-Goodall (1968:177.) So, Jason, did you just forget this or was it simply inconvenient to include it into your reasoning?

It's a simple, basic observation. But because it favours the AAH you just refuse to accept it. Biased is the word."

Algis Kuliukas in sci.anthropology.paleo Oct 21 2005

How one can say this "favours the AAH" as Algis has for years now (from July 2004 until the present -- 2009) is a mystery. Walking on wet ground is *terrestrial*

walking; when you walk through a park on a rainy day you are *not* engaging in non-terrestrial walking. And of course walking on wet ground cannot, in any sensible scenario, provide selection pressure to give us the features of fully aquatic mammals. This is reaching into the loony territory shared by people who've emailed me to say that one proof we were aquatic is that I have water piped into my home. It's almost as if anyone (like Algis here) making such a claim is deliberately setting out to discredit the AAT/H, and I have to admit they're doing one heck of a job.

References

K. D'Août, E. Vereecke, K. Schoonaert, D. De Clercq, L. Van Elsacker and P. Aerts

2004 "Locomotion in bonobos (*Pan paniscus*): differences and similarities between bipedal and quadrupedal terrestrial walking, and a comparison with other locomotor modes" in *J. Anat.* 204, pp353–361.

Duchêne, V

1997 *Inleidende studie van de locomotie van Pan paniscus: de bonobo als model voor de evolutie van menselijke bipedalie*. License thesis, University of Antwerp.

Hunt, Kevin D

1994 "The Evolution of Human Bipedality: Ecology and Functional Morphology" in *Journal of Human Evolution* 26: pp 183-202.

Kuliukas, Algis

2001 *Bipedal Wading in Hominoidae past and present*. M. Sc. dissertation, University College London.

Videan, Elaine N. and W.C. McGrew

2002 "Bipedality in Chimpanzee (*Pan troglodytes*) and Bonobo (*Pan paniscus*): Testing Hypotheses on the Evolution of Bipedalism" in *American Journal of Physical Anthropology* 118:184–190.

Algis changes Sharp and Costill's possible 3-4% into 12-13%:

"Like citing Sharp & Costil as if it gave evidence that speed was hardly effected in shaved competitive swimmers when, actually, the experiments were same speed so *of course* there'd be hardly any difference in speed and, actually, the push off and glide experiments showed a 12% reduction in decelartion after shaving." [19 Apr 2004](#)

"Bottom line, Sharp & Costil showed that deceleration after push off and glide was almost 12% less in swimmers that had shaved body hair at 2.0m/s. 12% less is *massive.*" [19 Apr 2004](#)

"The figure I think you were thinking of is actually about 12.9% (difference in deceleration at 1.5m/s after underwater push off and glide)

13% benefit in one swim is massive, Michael." [3 May 2004](#)

"If it was 'psychological', Bob, how come the Sharp & Costil paper found almost 13% decrease in deceleration in push off and glide experiments?" [5 May 2004](#)

"But as even a male human swimmer doing a push-off and float experiment exhibits nearly 13% decrease in deceleration after shaving..." [6 May 2004](#)

"You say that, but even a male swimmer can get almost 13% benefit in a single push-off and glide from shaving his off body hair." [9 May 2004](#)

"If a man can improve drag reduction whilst moving through water by 13% simply by shaving, I think it's a plausible hypothesis that something like this kind of benefit was part of the adaptive benefit of our own nakedness." [9 May 2004](#)

Summary

Unlike some, I really think of -- and treat -- the AAT/H as a theory (it is correct that actually it shouldn't be called a "theory", but rather a "hypothesis", but I think that's not a critical issue in a lay discussion). Although Morgan and others have cried out for it to be treated as a legitimate theory, they don't seem to me to really mean it most of the time. They seem instead to simply want it to be accepted, but that's not science, that's belief systems. Legitimate theories get questioned and hammered away at, and frankly it gets a whole lot tougher "out there" than it ever gets "in here".

If the proponent(s) of a theory accept the challenge of scientific critiques and address the questions and objections that are raised, their theory improves. What I've generally seen done by AAT/H supporters is a reaction that usually consists of saying the same thing over again (and "shooting the messenger") without seriously addressing the objections. Do that with any theory and it turns to mush.

Internal inconsistency

The AAT/H is a theory which seeks to replace well founded and well argued theoretical views which have been made for many years. It is also radically different. I think it is reasonable to expect that for such a radically different theory to replace all others it should not be "supported" by the sorts of sloppy and/or dishonest methods I've described here. It has been, in print, in all its incarnations; you have only to check out my examples (and keep in mind that my examples are not an exhaustive list -- far from it).

Besides the many false "facts" it proffers as evidence, the AAT/H arguments are a hotbed of examples of the Logical Fallacies, including ad hominem, ad hoc, Strawman, Irrelevant Conclusion, Fallacy of Exclusion, Fallacies of Ambiguity, Appeal to Pity, and Special Pleading. It claims to use the principle of convergent evolution, but does so in a strange and unscientific way: first, they use it only in cases where they want to (inaccurately) claim similarity of features, not when it is evidence against their theory (as in the case of the actual ubiquitous aquatic features); and their version of convergence ignores the role of phylogeny (relatedness) in evolution (as in expecting humans to use reptilian or avian salt glands for salt excretion instead of kidneys as *all* mammals do). Since phylogeny is the central idea in evolution, this places their theory not only outside the mainstream, but indeed outside of all evolutionary theorizing for the past half century or more.

Another major problem the AAT/H has arises from its ad hoc reasoning: the theory is internally inconsistent. For instance, the aquatic ape needs to be in salt water to evolve its radically different salt excretion system, but it needs massive amounts of fresh water for this system to be even remotely possible. Babies need to be aquatic to explain their fat, and non-aquatic to explain their larynxes; later, as children, they need to be non-aquatic again to explain their lack of fat and sebaceous glands, and aquatic to explain their larynxes. Women need to be far more aquatic than men to explain their fat and hair differences, but men need to be far more aquatic than women to explain their sebaceous glands. Although these things are contradictory, generally AAT/H proponents use them all at once, or all in the same book or article. This means the theory is not only contradicted by facts, but by its own claims.

Why is the AAT/H so popular?

The AAT/H, I hope you've seen, suffers a number of problems that render it incredibly unlikely to be true. The evidence which is supposed to support it is either chock full of "false facts", or consists of misrepresented or misunderstood science. The style of argument used in putting it together contains so many of the classic logical fallacies the theory as a whole makes a good case study for students of the

history of science, whether in anthropology or other fields. So an interesting question remains: why do so many people fall for it?

No doubt a major reason is because people like a good story. The idea of an aquatic phase appeals to a lot of people as a story, and the tale of a theory championed by outsiders showing up hide-bound scientists who conspire to hide the truth is a classic. But for a good story to be good science, it has to use actual as opposed to false facts, it needs to not contradict basic, established physiological principles, and it can't go misrepresenting what scientists say. As for how good a story the AAT/H is, I think real science is a far more fascinating story, and I think it's pretty mean-spirited of AAT/H proponents to take advantage of the public's lack of knowledge about the subject.

Part of the problem is that most people who like the AAT/H know little outside their own personal areas of expertise, so they accept the AAT/H proponents' version of the facts, and a typical AAT/H argument uses, common to other fringe ideas, "snowstorms of details to give the impression of vast knowledge" (John Cole, "Cult Archaeology", *Early Man* Spring 1980; this method is also seen in creationist arguments, where it's known as the "Gish Gallop" after a famous practitioner who flings out enormous numbers of false "facts" as an opening gambit in debates). If you're confronted with a list of features which are unique to aquatic animals and humans, it seems sensible that this should be explained. If it's then stated, quite accurately, that paleoanthropologists' theories don't explain why these features are unique to aquatic animals and humans, you might agree that an alternative to their theories is in order. The essential problem with this, as this site shows, is that these AAT/H proponents' lists of features are hogwash. This means that there's a really good reason why paleoanthropologists' theories don't explain these similarities -- they can't reasonably be expected to explain that which isn't so. The fact that the phony evidence for the AAT/H is drawn from so many widely varying fields means that few people, even those with scientific expertise in one area, will have much knowledge of them all.

The unpopular role of luck in evolution

Some people may like the AAT/H because they don't see how we would otherwise have been *forced* to become what we did become; the hypotheses coming from academics don't have this element of being compelled to do so. This is a major thrust of Morgan's arguments, for one. I think this is because many people are uncomfortable with the concept of luck in evolution, but it plays an enormous part. Sometimes luck in evolution is as bald as being in the wrong place at the wrong time (see paleontologist David Raup's 1992 book, *Extinction: Bad Genes or Bad*

Luck? for an exposition of this problem); a localized population, a big meteor, and poof! no matter how promising you might have been, it's over, you're extinct. Another way this happens is that of the changes that happen now between isolated populations in a species, some will turn out to work way better over time even though all work well enough now. What happens is that different populations of essentially the same animal (we'll just talk animals here) vary. This is always the case. Sometimes these differences become exaggerated, but each variation actually works well enough to last. But some changes allow other changes as well, which in turn allow other changes later, and so it goes. This sort of thing is exactly what we have in our past.

We have as our closest relatives the African apes, and both we and they have changed over time. At one point we were all one species, but various populations took slightly different paths. Of these changes, the most dramatic early change has been in locomotion; we used bipedalism more, and they adopted a particular form of quadrupedalism called knucklewalking. For an extremely long time, some 4-6 million years, both systems worked quite well. Both chimps and gorillas were likely as numerous as hominids (they remained fairly numerous until modern hunting and habitat destruction caught up with them in the past 100 years or so). But our adaptation to bipedalism also coincidentally allowed for other, later advantages, for instance freeing the muscles of the diaphragm from the burden of locomotion, which wasn't of any immediate advantage (so our ape cousins didn't suffer from not having it) but which later allowed other lucky changes to be useful. For instance, in the bipedal branch of the family, due to that earlier coincidental freeing up of the diaphragm, an improved larynx, along with a hyoid bone which descends with it, would allow for enhanced communication, an advantage which would mate up well with our existing primate mapping and problem solving abilities. This would then create a feedback situation, where both improved diaphragm control and larynx would be an advantage to this population of hominids, and these features would also make larger brains more useful. This also adds to this feedback where one physical feature makes improvements in another an advantage, which makes improvements in the first feature an advantage, and so on. Changes like these ended up giving us a huge advantage over our relatives after 4-6 million years, even though the initial changes were just different and not necessarily a huge advantage. But their changes, equally useful at the time, remained perfectly useful for millions of years -- just not so useful in so many places as ours later turned out to be. But being on one track or the other at the start was just a matter of luck.

Parsimony and Occam's Razor

Another reason people seem to like the AAT/H is that it's said to be "simple". This is a common comment I've heard about it, sometimes stated using the science term parsimonious. Occam's Razor is sometimes brought forward to support this "simplicity" as a valid reason for accepting the AAT/H. This is usually stated in the manner of: "Occam's Razor reminds us that the simplest answer is the correct one". But not only is the AAT/H not simple or parsimonious, that isn't even what Occam's Razor says.

Perhaps the primary reason the AAT/H is so often thought to be simple or parsimonious is that, again, people tend to accept the AAT/H proponents' version of the facts. It sounds simple that one aquatic period explains so much, and for students, this is attractive cause there's so much less to study. For a feature like -- to pick just one as an example -- sebaceous glands to be numerous in humans ("like the seals") due to an aquatic period seems simple and direct. One step -- but it's only one step if we accept the false "facts" the AAT/H gives us. The reality is that if it *were* due to an aquatic period both the glands and the skin would have to have changed radically from the ancestral condition to what we see in aquatic mammals (sebaceous glands present equally in both sexes and functioning at a very early age, and a radically different sort of skin surface) and then change again to what we see in humans (sebaceous glands present predominantly in males and not functioning until puberty) and this second change would have to be so complete as to leave no trace whatsoever of the skin change. That's *two* massive changes, as opposed to the paleoanthropology view that this is an attenuation of what we see in other primates. This is just one example, mind you; I'd have to repeat much of the info I've already got down on this site's pages to list all such examples.

About the common misconceptions of what Occam's Razor says, let's look at the sci.skeptic FAQ:

Ockham's Razor ("Occam" is a Latinised variant) is the principle proposed by William of Ockham in the fifteenth century that "Pluralitas non est ponenda sine neccesitate", which translates as "entities should not be multiplied unnecessarily". Various other rephrasings have been incorrectly attributed to him. In more modern terms, if you have two theories which both explain the observed facts then you should use the simplest until more evidence comes along.

This doesn't mean it's correct or even likely to be correct, just that you probably want to go with it until the evidence favors something else. The problem for the AAT/H is that the evidence has *never* favored it, even though AAT/H proponents have said it did, and even though some, like Morgan in her latest book, have issued *mea culpas* which claimed the evidence *did* favor some points but that more evidence came along and invalidated them. When you look at the evidence, this

just isn't true; virtually on every point the actual evidence invalidating the AAT/H argument was available *when the AAT/H argument was first made*, and in far too many cases, it was available *in the very source the AAT/H proponent used to try to support their claim*. This is an extremely serious error, if error it is, and calls into question the trustability of the authors. In science, it's okay to be wrong, but really bad to not be trustable. When I look at the litany of "false facts" the AAT/H has left scattered in its wake, the feeling I have is that the authors of the theory can't be trusted in their research. Unfortunately, I'm afraid that's going to be the longest lasting legacy of the AAT/H: a trail of "false facts" that, as Darwin noted, last very long indeed.

Vagueness

Scientific theorizing is typically done in exacting language -- almost too matter of fact at times -- which is intended to make what the theory says extremely clear. In contrast, AAT/H proponents commonly seem to have difficulties with fully laying out just what their theory says, except for intimating that somehow, in some unspecified manner, some vaguely specified degree of contact with water produces changes in fat, hairiness, sweat glands, etc. That it hasn't done so in things like bipedalism and sweating (etc.) for actual aquatic mammals is left unexamined. That the only aquatic mammals that fit most of their other criteria are whales, sirenia, and seals (except that seals aren't hairless, including tropical seals) is also left unexamined, and anyone who points this out is accused of misrepresenting what AAT/H proponents claim -- yet they are the ones who claim humans have these features shared only by those fully aquatic mammals. They don't seem to realize the implications of what they are actually claiming -- either that or they are deliberately obscuring what they are claiming. As I've said many times, I'm not a mind reader, so I can't tell whether they are honest and incredibly poor researchers or deliberately misleading others -- maybe they don't know themselves anymore.

Theory leading the data

Finally, this quote from a newsgroup describes an essential problem with the AAT/H:

If you put discovery of the mechanism of evolution first, it means that you have to wait on a convincing trail of artifacts, skeletal material and/or, lately, genetic material, first. If you put the theorization first then you simply have to rearrange the existing material, edit out what is

inconsistent, twist and turn the facts, and Walla you have a 'why' theory that no-one can absolutely disprove. Of course you also have a theory that is also beyond actual proof.

One common feature how the theorist actually behave, in an instance whereby they make a claim, it is often based on the lack of evidence they use as evidence for their theory. When the evidence for something arises, then they twist the theory such that lack-of-evidence that previously supported their theory becomes the presence of evidence that supports their theory. Negative of positive, the evidence always supports the leading theory. Why else do you create a theory before you have sufficient evidence, so that you can lead the data as it comes out, at least until so much data comes that the theory loses all support. Very seldomly does the theorist actually back out of supporting his theory because new data fails to support it.

An excellent example of this are the C13/C12 levels in africanus, as it became increasingly clear that africanus was a grassland derived carbon consumer, the AAT theorist tried to maneuver their theory such that it had to be an aquatic grassland that was providing the C13/C12 levels, and the focus then became, see C13/C12 levels support aquatic theory because sedges are also aquatic and thus Africanus must have consumed sedges (not might have, may have or could have, but must have).

Philip Deitiker, 22 Sep 2004 sci.anthropology.paleo

In the hands of AAT/H proponents it typically ends up with this formulation:

- a) if A is true, then the AAT/H is true, but
- b) if A is untrue, then the AAT/H is still true.

An example (one of many which could be made; Deitiker gives another in his quote) is behavior toward water among apes.

This in fact was also the essential core of the response to the info I posted some 8-10 years ago about chimps' responses to water and their commonly supposed hydrophobia. Their supposed hydrophobia was taken as evidence for the AAT/H, but when I pointed out papers describing that this supposed hydrophobia was just not so (they vary a lot in their responses to water, just as they vary in many other habits and likes/dislikes) that also was taken as support for the AAT/H. This was amplified when AAT/H proponents began dealing with information about bonobos, chimpanzees, and gorillas wading, information that had been known in anthropological circles for some time but which the AAT/H's proponents didn't find

until later (the gorilla info not, apparently, until *National Geographic* did some articles on it).

And more recently this method, or tactic, was seen more recently on the subject of swimming by chimpanzees. It's pretty well known that chimpanzees don't like to swim and don't seem to be good at it; many times when a chimpanzee gets into a situation where swimming is necessary they simply drown. This has given rise to a "well known fact": chimps are physically incapable of swimming. You can find this "fact" stated baldly pretty frequently, although if you look online you'll see that the more thoughtful will state something different, far more nuanced, like [Jane Goodall](#) who says:

In general chimpanzees do not like to swim. Chimpanzees have stocky bodies that prevent them from being strong swimmers. Many chimpanzees, however, enjoy splashing around and playing in water.

or the [San Diego Zoo](#):

Chimpanzees don't like to be in water and usually can't swim.

or even the online [The Big Zoo](#):

Chimpanzees avoid large bodies of water and are usually only able to swim if extremely excited.

And in fact, at the very least, we find one chimpanzee swimming -- David Attenborough mentions him in a 2002 book, *"Life of Mammals"* (in what you'll see later is irony, I first learned about this through a newsgroup post by AAT/H proponent Algis Kuliukas, although several years after he'd written it). I've seen it stated that others have seen common chimpanzees swim (very rarely) but I don't have a single reference for this, so you can't take that definitively... but here's the thing: even if that one chimpanzee that Attenborough mentions is the one chimp in all of chimpdom for all time who ever swam, it still proves that chimpanzees are *capable* of swimming. (By the way, you may also find statements about various other animals not being able to swim. I've seen this claim made of rabbits, giraffes, kangaroos, sloths, bats, and camels, and almost all these claims are false (giraffes, as far as I have been able to find out, cannot swim due to their physical build; as you might imagine, in relatively calm water they wade very well at considerable depths). If I was forced to wager on how many mammals are physically incapable of swimming, I'd bet none except for giraffes and, oddly enough, adult hippos. (Adult hippos are the only semiaquatic mammal which cannot swim. The apparent reason is that they are very dense.)

Now this may seem like splitting hairs, but when you want to figure out why something is the way it is, you have to first be accurate about the way it is -- otherwise you *will* wind up with nonsense. This is the "garbage in, garbage out" axiom common in computer programming (and is also seen, for instance, when people ask why humans lack body hair when we don't -- you need to ask about *the actual condition or characteristic*, described accurately, even painstakingly accurately or your answer *will be wrong*). In the case at hand people have worked to answer the question "why *can't* chimpanzees swim?" when they really want to be answering "why *don't* chimpanzees swim?" and this means they come up with a wrong answer -- there's no way they couldn't come up with the wrong answer. The usual answer is that chimpanzees are too lean, and therefore dense, to be able to swim. Now, even without the evidence of the chimpanzee swimming, this could be seen to be false, with just a little thought and study. First, one might realize that jockeys, noted for having extremely low -- often unhealthily low -- levels of fat, can swim, and often do as recreation or as part of their training. But it's not just jockeys; one of the 1990s best swimmers is noted for having a very low level of body fat -- 3% -- which puts him right in the chimpanzee range. His name is Tom Dolan and he's a remarkable athlete, not just because of his body fat, but because he was a 5-time U.S. national champion, world record-holder swimmer -- an Olympic gold medalist in 1996 (with his picture on a Wheaties box to prove it :) -- in spite of having asthma and tracheal stenosis, which is a narrow windpipe (20% narrower than usual). He shows that the dense/body fat explanation doesn't make sense. And then along comes a chimpanzee to nail down the proof.

But all this, while interesting, is just trivia as far as this site is concerned; what makes it apropos is how this information has been used by AAT/H proponents. Not the Tom Dolan info; they never looked up that apropos information even while saying that body fat explains chimp non-swimming. What I mean is the "chimps cannot swim" idea.

Previously I mentioned Algis Kuliukas, who has become one of the AAT/H's chief proponents. In September of 2001 he emailed me after seeing my site (he didn't like it) and in this email he said flatly that chimpanzees can't swim and that it wasn't a matter of preference or behavior but rather a physical impossibility. He considered this powerful evidence for the AAT/H. Then in November of 2002 he read Attenborough's book and learned that chimpanzees *could* swim, which he said changed his perspective. "The distinction in swimming between humans and apes would certainly not be as distinct as I had thought..." he went on, and since chimps being unable to swim was so important a piece of evidence *for* the AAT/H (his "mild" wading version), one might reasonably expect that he'd feel the opposite evidence would be evidence *against* it, right?

Wrong. He continued that sentence with "... although I feel this data only adds still further to the plausibility of wading as a model for bipedal origins."

- a) if A is true, then the AAT/H is true, but
- b) if A is untrue, then the AAT/H is still true.

Or as Philip Deitiker said, "When the evidence for something arises, then they twist the theory such that lack-of-evidence that previously supported their theory becomes the presence of evidence that supports their theory."

On this particular bit of data, there's an extra interesting AAT/H method involved, and harks back to my "What is the AAT/H?" page and this section: "When honest scientists have facts pointed out to them which prove their line of evidence is wrong, they drop that line of evidence. AAT/H proponents rarely do, and even when they do, others -- both active proponents and casual aficionados of the idea -- often continue to use it."

Here we see Kuliukas using a fact he thought was true in 2001 (and using the "true" position as support), and learning it was not true in 2002 (but using the "not true" position as support anyway); but after that he begins again claiming they can't (not "don't", "don't like to", "do so extremely rarely", but "can't"):

"Put it this way, really what's the explanation that chimps can't swim?"

Kuliukas sci.anthropology.paleo Jan 1 2004

"Moore et al think humans evolved without any significant influence of water even though we can swim but chimps can't."

Kuliukas sci.anthropology.paleo Apr 23 2004

And he continued to do so for many months until I pointed out his hypocrisy, after which he started waffling something fierce, first making up duration times and distances for the Attenborough chimpanzee swim and insisting that since chimps (rather obviously) do not swim as well as or anywhere near as frequently as humans, "chimps cannot swim", then turning the account into a "hypothetical chimp". He could have said any number of accurate things: they don't swim well, they don't swim often, or even they don't seem to swim except extremely rarely and under unusual circumstances. Instead he simply refused to do the more nuanced sort of position we see in the Goodall and San Diego Zoo statements I mentioned above, insisting on a binary can/can't construction that was contrary to facts he knew, to the point where he finally ending up insisting (despite the record of what he'd written):

"I never claimed it was impossible for chimps to swim but they're clearly not as good at it as humans."

Kuliukas, sci.anthropology.paleo May 26 2005

The point here is not the personalities involved but rather the AAT/H method this example shows, because it's common:

1. a) if A is true, then the AAT/H is true, but
b) if A is untrue, then the AAT/H is still true.
2. Claim something based on lack of evidence ("A is true") then, if "evidence for something arises", showing that "A is untrue", "then they twist the theory such that lack-of-evidence that previously supported their theory becomes the presence of evidence that supports their theory."
3. Afterward continue to use the lack of evidence as support even though they know the evidence exists until...

Someone points out the hypocrisy involved, at which point the AAT/H proponent either waffles, trying to continue to use the "A is true" position even though they know it's not true, claiming they'd never said what they can be shown to have said, or possibly, as we saw in Verhaegen's approach to my pointing out he was wrong about [Steller's sea lion pelage](#), acknowledge the correct info and then afterward simply confine the incorrect statement to audiences likely to be more friendly and less questioning. Or -- again from Verhaegen -- simply declare (inaccurately, of course) that any evidence you like is evidence, while evidence you don't like is dismissively waved away as "non-evidence", as he did with ["auditory exostoses"](#): *"The presence of AEs proves diving habits, their absence does not."*

That's not the way to do science.

This desire to have it both ways, or all ways, so that any evidence is evidence for the AAT/H and contrary evidence is wished away, is one of the most damaging things that AAT/H proponents do to their attempts to make a reasonable theory. (That, and shooting the messengers who help them out by pointing out their many errors. :)

Objections and answers

This eclectic list of objections to critiques of the AAT/H was drawn from various forums and newsgroup postings. They illustrate, I think, some common misconceptions which make it harder for many to understand the counterarguments to the AAT/H. Amongst the AAT/H principle proponents only Elaine Morgan is represented here at this point; I'm sure as I write more there will be more from the

principles. Except for the objections of these principle AAT/H proponents and professionals who should know better, I've deleted the names and initials of posters. Some of their objections are well thought out but naive, while others are simply foolish. My aim is to point out the inaccuracy of their objections without embarrassing them. All the writings of others here are in blue

My opinion? Anthropologists should study humanity as it exists now, and leave the question of when and how it arose to those more qualified, like evolutionary zoologists and molecular biologists...

Anthropologists *are* qualified to study human evolution; it's a major part of the field. A great deal of the study of human evolution is cross disciplinary, needing either study of or input from many different fields. In fact, one of the problems commonly seen in studies of human evolution when non-anthropologists do it is not spending enough time understanding "humanity as it exists now", both physically and culturally, which can lead to naive mistakes about what "we" are like, because often only a relatively small subset of "us" is examined. Anthropology itself is a big field now, where too many don't interact enough with their colleagues in the other sub-fields. Still, anthropology -- since it covers "the science of us", which is a pretty large subject -- is unusual because it does contain such wide-ranging sub-fields, which include not only cultural and physical anthro on modern peoples, but also physical anthro involving old bones and fossils, molecular anthropology, archaeology, and primatology. These fields also bleed out into other disciplines, such as linguistics, history, history of science, and psychology (in fact, in Great Britain primatology is generally found in the psychology departments).

The psychology and sociology and meta-science of those against the AAT never ceases to amaze me. Ms. Morgan is a grandmother. I can picture her like Andy Rooney composing her books on an old Underwood, consulting notes on 3 by 5 cards. It would be rather a burden for someone who works in that style to type in ALL the references. Not like appending a text file to a usenet post.

The most thorough researcher I've known was a grandmother, and she wrote everything in longhand in pencil on yellow legal-sized pads (*had* to be legal-sized); it didn't stop her from giving refs and double checking the primary sources for her claims. Regardless of the accuracy of the picture of a career television scriptwriter doing her writing (and Internet postings) with a manual typewriter, the issue is whether someone who is producing a theory which is intended to replace well researched and supported science should be able to get their view accepted when

it's supported by "false facts", misrepresentations of valid research, and altered quotes. Science not only doesn't work that way, it *cannot* work that way. And you don't get a free pass cause you have grand kids.

On the perceived need to always be near drinking water:

Our ancestors **MUST** have been guzzling lots of fresh-water wherever they were. In fact, because we lose so much water when we sweat, it is untenable to suggest that our ancestors were ever very far from water-holes of some sort.

First:

Define "very far"... then consider this: in the very recent past modern human hunters from gathering-hunting groups in arid and semi-arid regions (such as the !Kung in Africa and Australian aborigines) commonly did not carry water in their travels while hunting, and they traveled quite a ways; miles from water holes, certainly. Water can also be gotten in many ways other than from water holes or streams, for instance from plants or by sponging it from depressions in trees or rocks (both methods are used by both humans and chimps). The early hominids lived in less dry areas than these groups did.

The !Kung have high technology. They survive in an area where early hominids could not and they have ways of obtaining water far beyond the means available to early hominids. It's a mistake to use analogies with highly developed h.s.s. societies.

True, compared to our earliest ancestors they have very high technology. The point is that they didn't use any of their relatively advanced technology in using water during their travels; they don't use any technology at all! It negates the claim that early hominids would have to stay right by fresh water, because having fresh water with them even in an extremely dry desert environment is not necessary for humans today, as the !Kung demonstrated. This experiment by analogy is a valid one; it involves no technology or any developed or "advanced" technology or social structure of any kind.

On the idea that a bare skin would be too great a detriment for a terrestrial hominid:

Have you ever tried walking/running barefoot and naked through the woods? Poison ivy would be the least of your problems. An animal in that environment would have rapidly acquired a sensible coat of hair - even if it didn't have one at the start.

Since in fact anatomically modern humans ("us") did exactly this -- "running barefoot and naked through the woods" -- for tens of thousands of years, and since earlier hominids did so for hundreds of thousands and even millions of years before them, we know that this objection is invalid. Actually, this one is foolish, since even a moment's reflection would be sufficient for virtually anyone to realize this.

Where did our ancestors get the extra Omega 3 fatty acids needed in the diet for brain growth? From the marine food chain, perhaps? I'll give up on this if you manage to find one human being with a normal, functioning brain who has never eaten any seafood.

This has to do with the dietary significance of various fatty acids, such as DHA, which our bodies must either get through food directly or synthesize from linolenic acid (LNA). These acids are often mistakenly said to be only available in fish oils, especially those of fatty cold-water fish. (Looking up accurate information on this on the web can be difficult, since you have to sift through many sites devoted to selling you their fatty acid supplement, and they, for some reason, tend to "overstate" their case.) However, although fish are an excellent source of these fatty acids, wild game is also good (and this was, after all, the only type of meat available to our ancestors); also, sufficient quantities can be gotten through our bodies' synthesizing DHA from LNA obtained from plant sources. The Mayo Clinic points out that the amount necessary for modern humans is not terribly high ("One tablespoon of vegetable oil easily meets your daily requirements.")

It should also be noted that human infants are not as good as adults at synthesizing DHA from LNA, but this isn't a problem for breast-fed infants since human breast milk is also a good source of DHA. It shouldn't really have to be pointed out that amongst our early ancestors, all infants were breast-fed.

The particular AAT/H proponent objection above (in the words of humorist Dave Barry, "I am not making this up") is, sadly, not atypical in its lack of forethought. It should be fairly obvious, with just a bit of thinking, that during our evolutionary history, a great many humans have lived, grown up, and developed normal brains while living in environments where fish (and especially cold-water sea fish) were just not available, and so weren't eaten. To give an example that folks may be more familiar with, milk is a terrific source of calcium, but calcium is also found in a great many plants, and it isn't necessary to drink milk to get it. Same situation with these fatty acids; fish is a terrific source, but you don't need to eat fish to get them.

After a poster asked why AAT/H critics spent so much time pointing out mistakes and misrepresentations in Elaine Morgan's off-line and online writings; one such critic replied: "Primarily because she does present herself as an 'expert' and the AAT as a viable option." The poster then asked:

So why does this bother you?

My reply was:

Because she presents a theory and supports it with false "facts" and unsupported statements, then complains -- loudly -- if her work is ignored by professionals. When her work is examined and her research is found wanting, she complains about that too.

If it's ignored, people might reasonably think she actually has done reasonably good research and is presenting accurate facts. It is the process of science -- the essential core of science, in fact -- to examine theories and check out the facts they are supposedly based on. The question is, I would think, why does it bother so many people here when this essential requirement of science is applied to what is supposed to be a scientific theory?

This following poster asked a good question about how to gauge the accuracy of what you read online (he was talking about newsgroup posts in '96, but the same problem always exists on any post or web site, as well as in writings off-line and in conversations or presentations):

There is one slight problem, how do I judge the quality of information from the people who claim to be professionals? In my field I do see inaccurate information from professionals all the time. I see no reason a priori to assume that professionals are any more or less accurate than are electronics professionals.

It's a problem, I'll grant you, but science writing (and biography writing, etc.) deals with this by offering a means to check them out. This is why, in science, people give references for statements they make. If the references are checked out (and I would argue that this doesn't happen enough) and they have been reported accurately and actually support the claim(s) made, you have additional evidence that the person's statement(s) can be accepted as being valid.

Now, given the nature of this medium [newsgroup or forum posting], it's harder to do this here than when writing a book. But posts can help you determine -- not always perfectly -- who is a more reliable or credible source. If, for instance, a poster continually repeats information that is contrary to established fact -- as several posters did here when they claimed that shellfish are not high in sodium --

you should start to wonder how accurate any of their info is. If a poster states, as several have here in the past, that they do not intend to read anything on the subject but insist that their view is as valid as someone's who has read on that subject, you should start to wonder how accurate their views are.

As I mentioned, I don't think refs get checked out even in professional science as often as they should be; partly this is due to time constraints and partly because scientists tend to not think someone will, for instance, claim a reference says one thing when it says the exact opposite.

But if the references are checked out and the person turns out to have claimed something that is not supported by the reference and/or is contradicted by facts -- as happened when I checked out Morgan's claims about seals' "eccrine sweat-cooling" -- you can see that the original claim was bogus. If the references are checked out and they say exactly the opposite of what the person said they did -- as happened when I checked out Morgan's claims about hypertonic sweat (and last summer ['95] on several of her claims about salt and salt appetite) -- you can see that the person making the claim is either an incredibly sloppy researcher, or has incredibly poor comprehension of what they read, or is simply being dishonest. You can't tell which, necessarily, but you can tell that their work and statements should be treated with extreme suspicion (at best).

A similar instance which bears on credibility would be a poster making a claim, and supporting it with a reference, but avoiding any mention of contrary information also from the same reference. One instance of this was seen with Morgan's cite of William Frey's book on tears, where she mentioned only the aquatic animals and left out all the terrestrial animals also mentioned in that book (doing this through honest omission would require inadvertently skipping every couple of paragraphs in the applicable chapter). Another is her contention that salt glands do not exist in terrestrial reptiles and birds; she supported this bogus claim by citing Schmidt-Nielsen's book, *Desert Animals* (and ignoring posts with references that gave her the correct info). But in Schmidt-Nielsen's book he specifically states that these glands do exist in terrestrial reptiles and birds.

Clearly if someone has been shown, as has happened with Morgan in these cases, to ignore contrary information that is found in the same book, or even, as in these examples, *on the same page* as other information they use, that is the mark of a writer whose word cannot be trusted.

The same applies to some of the posts of the professionals. By the way I tend to believe the professionals on the "eccrine..." However, how do I know without checking the references myself that you are reporting them in an accurate manner?

Checking them is the most sure method, but you could also see if someone posts contrary info with solid backup. If they don't, when not doing so destroys their claim, you can reasonably expect they don't do it because they can't. This is the usual (messy but interesting) way science progresses.

Now, since you say that some of the "professionals" (they're not all pros, you know) have done what I talked about in the preceding paragraph, you must have examples. I would appreciate hearing what they are. Since you have provided no info to back up your claim (just as in the previous time you made such a claim) that such examples exist, your claim seems, quite reasonably, to be not credible.

This quote is from a newspaper column, written by Jay Ingram, a science writer and host of a television show on the Discovery Channel in Canada:

[I'd be unhappy if it faded away. Theories like this add color, wit and imagination to the scientific enterprise.](#)

Well, I think J.R.R. Tolkien's Middle-Earth works are colorful and full of wit and imagination but I don't think they should be taught as history.

What the AAT/H adds to science is a generous helping of false "facts" and poorly drawn analogies; this doesn't add anything useful to science. Sadly, if the proponents of the AAT/H had done their research in even a basic, simple, halfway rigorous fashion, they might have been able to present some valuable insights to science. However, since such basic, simple, and even halfway rigorous research would've shown them their evidence is inaccurate, they wouldn't have a theory to present. Just because something is a good news story, it doesn't follow it should be taken as good science. For someone in the business, a good news story is gold, and you hate to see it go, but in science news, you have to accept it, because if bad science doesn't "fade away", science doesn't improve. Even a science writer on the lookout for a good story should appreciate that.

And here we hear from Elaine Morgan with objections:

[EMorgan: Most of our disagreements are on matters of terminology, and interpretation, not substance.](#)

Our disagreements are not on matters of terminology, but on an extremely substantial issue: I think one should attempt to state facts instead of falsehoods; you, as we see above in this post [it was the info about salt glands in terrestrial

birds and reptiles], do not. You have no apparent qualms about claiming that researchers have said the opposite of what they actually did say (we've seen this with your recent claims about Hancock, Whitehouse, and Haldane's statements on sweat, and before with your claims about Derek Denton's work on salt and salt appetite).

I think this is a substantial disagreement, about as substantial as you can get, and it has nothing to do with either terminology or interpretation.

This same tack again later:

EMorgan: It seems to me that we disagree hardly at all on the facts; we disagree on how they are to be interpreted.

But in fact it is *not* a case of simply disagreeing on interpretations, as I have said when you made this erroneous statement before. AAT/H proponents in general, and you specifically, make a great number of non-factual claims, and these aren't just ideas that are later shown to be false by new information. They are things which are known to be false at the time they are stated, and often shown to be false in the very sources AAT/H proponents use. We've had many examples of this demonstrated in this newsgroup over the past couple of years. It's not just you: this is true of Marc Verhaegen and Alister Hardy as well.

I'm afraid it's been quite clear we disagree *a great deal* on the facts, despite how it seems to you.

Then a different tack (perhaps playing to the gallery):

EMorgan: You keep trying to prove I am a deliberate liar. You keep failing. You seem to work on the principle that "if I say this seventy-four times people will think it is true."

I attempt to demonstrate that your research cannot be counted on to have factual content. Thanks to you I am repeatedly successful at doing so. I make a point of *not* saying you are a deliberate liar; however, since the only other explanation I can think of is that you are an incredibly poor and unreliable researcher, this is probably of small comfort to you.

Then another, on my posts pointing out similarities in the tactics of quoting and evidence gathering between AAT/H proponents and "creation science" proponents:

EMorgan: No, he is just trying to connect me to them in people's minds. He is trying to make them think I am a nutter.

I don't think that even the creationists are "nutters"; not the ones who make up the data and alter the quotes to prop up their theoretical musings. They simply want to establish a theory and are willing to do virtually anything to do so, so they make up data and alter quotes to suit their purposes. So do you. What I am doing is pointing out that you are doing this; if that makes you seem like a "nutter", may I humbly suggest you *stop making up data and altering quotes*. Your faults lie not in me, but in yourself.

And another new one (albeit on well-worn ground) wherein one of Elaine Morgan's supporters again suggests she's too old to be expected to do science properly.

She is eighty-one years old this year. Can you imagine what she must feel like when she reads the nasty rubbish you have written about her. Couldn't you have shown some sensitivity? What kind of person are you?

The kind of person I am is one who treats older people of both sexes as people; real people fully capable of thought and with the ability to do real scientific work. Frankly, it saddens me to see this attitude on the part of some of Elaine's supporters that suggests she is senile or somehow "past it". You should really get over this horribly prejudiced ageist attitude of yours.

My response to Elaine Morgan's response to me

Are we confused yet?

While surfing a while ago I came across a response Elaine Morgan had made to my site. Although in this response she makes some demands of me, she seems to have chosen not to actually let me know about them, but simply to post them on one of her supporters' web sites. (Maybe she didn't really want me to do what she insisted I do. But then that would make her demands just a grandstanding rhetorical device. Oh well...) Let me make it plain that although I critique Morgan and other AAT/H proponents here, I am not insisting they provide me with info or otherwise oblige me; I just wish they'd do even the minimum research required of any theory.

Since I found (not really to my surprise anymore) that much of Elaine's response was, sadly, error-laden, I've reprinted it here with my clarifications and corrections.

I kept their color-formatting, which had material from my site in green, and Elaine's responses in blue; my added corrections, etc. are in black.

JM:

Page 1

Aquatic Ape Theory: Sink or Swim?

Instead I've begun an ongoing response of pointing out errors of fact, errors in theoretical understanding (which, though critically important, is more problematic because a lot of people seem to think this is waffling), and urging the theory's proponents to respond to valid objections to their theory.

There are a number of claims made by AAT proponents, and tracking down the source material which supposedly supports these claims is not always easy. In a dramatic departure from the way science is supposed to work, references for specific AAT claims are not always available; many times the author(s) involved simply make a claim without any reference to a source of evidence for the claim. For instance, during the period I put together most of the evidence for this site, chief AAT theorizer Elaine Morgan was posting regularly in the sci.anthropology.paleo newsgroup, and claimed to be willing to supply references for her written AAT claims, but she proved to be reluctant to provide these references to people who have a past history of actually reading the source and reporting back what it really says. Others -- who didn't engage in this sort of "anti-social" behaviour -- reported they have no problem getting references from Morgan.

EM:

I supplied every single reference which Jim asked for to the best of my ability. Perhaps instead of this blanket claim he would like to specify which one(s) he is still looking for and in what words I expressed my "reluctance" to comply?

Only one request of his I failed to comply with. He instructed me to compile a list of every quotation I had ever used in any book or paper, and give details of the references including page numbers. I thought that was a bit much.

I did, after repeated requests, and many weeks of waiting, get a ref for one of two specific statements I had asked about, and Elaine posted the other after one of her supporters asked for it (she ignored my repeated requests for it). Both of those refs proved to not support her statements (they're detailed on this site, re seal sweat and re aldosterone and head-out immersion).

That last Elaine refers to was a request for "any and all" references, and I specifically said that any, any at all of her references for the statements in her books would be appreciated. This is a common and valid use of the English language, and means that I'll take any I can get, and that I'd love as many of them as possible (I do find it odd that having to explain this to an Oxford English graduate).

JM:

The more curious or enterprising amongst you may wish to read further about some of these features, and you'll find that easy to do by simply going to a library and picking up one of the books or journals I mention. If you try to do the same with most pro-AAT accounts, you'll find one of several problems: they often don't give a reference for a statement or quote at all when they do the reference is often incomplete (just a name with no clue as to what publication or year the info is supposedly from; sometimes a wrong name).

EM:

As Jim is too modest to mention, I took this criticism on board and in my last two books he would be hard put to it to find any quote that was not fully referenced.

Here again Morgan is either deliberately misleading or just not understanding -- after years of people explaining this to her -- what is minimally expected from works intended to build a scientific hypothesis. References are needed as support for statements (accurate refs that actually support the statement), not just quotes (although for quotes it is expected you'll provide page numbers as well so they can be checked for accuracy). In one manner or another, Morgan falls down on this in all of her books, although, as I've already said here on my site, she has done more of this in her last book than she had before. This, however, includes references that don't actually support what she's saying.

JM:

For instance, it took some hunting to find the source for a quote when the quote supposedly came from a "famous authority during a television programme" (it was actually a 1929 book by Prof. Frederick Wood Jones).

EM:

I took the quote from Desmond Morris. He didn't remember where he got it from and thought it might have been the Brains Trust. I later found the source in the 1929 book and included that information in a later publication which Jim has not read. Or else he has read it but prefers you to think he found it unaided and that I will be chagrined to see it unearthed. Why would I be trying to hide it? Jones said exactly what I said he said. Jim started out with the conviction that I was making up quotations off the top of my head.

I simply pointed out that you were wildly inaccurate and it made finding the reference incredibly difficult. I thought you'd just made up the TV reference; now you say Desmond Morris just made up the TV reference. Seems odd to me that Morris, who was after all in the TV business in London at that time, would remember this so inaccurately (yet verbatim) or that he would remember it verbatim for his book when the show had been off the air for years at that time. (Or for that matter, that Prof. Wood Jones could appear on a TV show that didn't start till a year after he'd died.) The point is that when you use a quote, you should ideally give the source, but if you don't know the source, you should not give the source anyway, because let's face it, how accurate is that likely to be? Not giving the source is the approach Morris took when he quoted Wood Jones in his book. If indeed Morris gave Morgan the erroneous info about Wood Jones' after-death "TV appearance", she really should've checked it out or simply not reported hearsay as fact.

p.s. I certainly never gave the impression that I ever had the conviction that Elaine was making up the quotation, just the source. She does have an ongoing history, however, of altering quotations to suit her needs, as documented elsewhere on this site.

JM:

So I guess I'd say my qualifications for this work are 1) a knack for library detective work, 2) an ability to learn basic scientific precepts (anyone should be able to do this one), and 3) being just a little bit nutty, cause those examples I just gave show you've got to be a little crazy to do it.

EM: You didn't actually give the examples Jim. You didn't pin down the errors. A short list of those would be more convincing than the harrowing tale of what you went through looking for them.

Elaine seems to be up to the sort of selective reading she uses in books like Denton's *The Hunger for Salt*, and Frey and Langseth's *Crying: The Mystery of Tears* (as described on this site). In this case she seems to have completely skipped virtually every page of my site. But of course it certainly can't be described as a *short* list.

JM:

As a note, let me state that I am not the Jim Moore who is a primatologist at UCSD, nor am I the Jim Moore who wrote and co wrote several books on Darwin, nor the Jim Moore who's an anthropologist at CUNY. Although I'm not them, from what I've seen of their work, I wouldn't be insulted to be mistaken for any of them. I hope they feel the same.

EM:

Dream on!

I've only heard from the primatologist at UCSD, and he seems to like it. He says of my site: "For a thorough and entertaining discussion of the theory that we went through an aquatic phase prior to *Australopithecus*, this is THE place to go. Erudite, witty, and insightful".

Good enough for me.

JM:

In surfing while rewriting and adding to this site, I've noticed several AAT boosters make the rather odd criticism that my previous site was old, as if that somehow made the facts therein wrong. Although many of the facts posted on this site were first posted on my old site in 1995, they are still valid. The facts have not changed. The diving reflex has been present in terrestrial mammals for centuries, it was in 1995, and it still is, despite the AAT claim to the contrary. Salt hunger has been present in humans for centuries, it was in 1995, and it still is, despite the AAT claim to the contrary. The infant swimming response has been present in terrestrial mammals for centuries, it was in 1995, and it still is, despite the AAT claim to the

contrary. Hymens have been present in terrestrial mammals for centuries, they were in 1995, and they still are, despite the AAT claim to the contrary.

EM:

What you don't point out is

a) that in every one of those examples, as evidence came to light that the sources I was using in 1972 were proved to be mistaken, I specifically amended them in subsequent publications, and drew attention to the fact that the previous statement was out of date. In every single case.

b) many of them were based on data given in papers by hard-nosed scientists and believed by them to be right at the time.

c) There is no scientist in this field whose work could not be shown in the same way to be suspect. In 1972 several of them believed our ancestors had lived on the savannah for twenty million years and been separate from the apes all that time. Jim could do us a great service by pointing out that they were wrong. What they wrote then has not changed. The facts have not changed. So they must have been lying...?

Sadly, this is not true. First. these weren't, as she intimates here, just said in her first book; she has repeated many of them up to and including her latest (1997) book. In some cases the actual, contrary, facts had been known for many years *before* she began writing, for instance, the facts about the diving reflex and the infant mammal "swimming" response were known for *decades* (!) before Hardy said his first words about the AAT/H (using bogus info about the diving reflex as support). Some of Morgan's claims are actually contradicted by the facts *in the very sources she claimed as support for them* (salt hunger and its implications for our ancestral environment, for instance). As I pointed out in my critique of Morgan's 1997 book, not only hasn't she dropped some of her bogus claims, she has repeated them there. As only a few examples, a quick browse through that section alone shows these problems on the subjects of hymens, swimming babies, fat vs. fur as an insulator, her strawman bogus savannah characterization, and of course her statements that sebum waterproofs human skin (it can't) and has no known purpose in humans (it's a scent producer -- pheromones, you know). There's info on each of these on this site (along with a lot more, of course).

JM:

There was a conference on the subject in 1987 with both pro and con participants, and a book from that, *The Aquatic Ape: Fact or Fiction?*, shows the relative

strengths of their positions. Although I'm sure any AATer would disagree, the pro-AAT articles in that book are distinctly amateurish compared to the ones which don't support the AAT. It's a good book to take out and read sometime if you can find it, as it does cover the basic arguments and has some good arguments to the contrary.

EM:

I would recommend it. I persuaded my publisher to print it. It is quite true that the style of the opponents is more professional. The opponents were professionals.

A) non-professionals can (and need to) do professional-quality work (see my mention of Alan Mootnick as an example of a non-professional who does); and

B) professionals can (but shouldn't) do non-professional-quality work (many of the pro-AAT/H contributors in that book are University-level professionals in scientific fields. They just didn't do the work professionally). Morgan is correct in admitting that the work of those opposed to the AAT/H was more professional.

JM:

P.S. One thing I found from posting on this subject in sci.anthropology.paleo is that Elaine Morgan

a) doesn't like it when you imply she is the primary proponent of this theory (she is certainly the best known and widest read)

and

b) doesn't like it when you mention any claims made by AAT proponents other than her.

EM:

a) I do (or did) get tired of reading that the idea "had been dreamed up by a Welsh TV-writer housewife" (guilty on all counts) rather than by an Oxford professor and Fellow of the Royal Society.

and b) What gave you that idea?

A) I have always said (many times, in fact) I don't care who says what, just that what they say is backed up by facts. Your work so far has not qualified.

B) What gave me that idea? It was one of the many invective-laden newsgroup posts you aimed at those who pointed out your errors for you. It may have been the one when I mentioned others' AAT/H claims and you said I was "just smearing like the worst kinds of politicians"; or the one where you said I was a "Joe McCarthy"; or it may have been the one where you said that my reporting these errors of facts (yours and others'), with refs to back it up, was just "all trumped-up imaginary statements made up out of your head. You really are a shocker." Or maybe it was all of these, and others.

Plus, you did it again in this very response. It's in your reaction to my response to the issue of sweat as a salt-regulatory system (you'll come to it as you continue down this page).

JM:

Another major problems with doing a good AAT critique is that the AAT's proponents generally insist that one accept the AAT view of what physiological changes did happen and which are relevant. So they only look at some, ignoring others.

EM:

What's this "insist"? Everybody gives his own view of what is relevant. And Jim would like you look at the others, ignoring the some.

I'm examining the things that AAT/H proponents say are relevant, like your books and your pamphlet's list. I'm trying to cover them all but it is time-consuming; AAT/H proponents put out an enormous number of statements, and since they do so without properly examining the evidence, they can really churn it out. All those I've covered so far have shown the pro-AAT/H view to be in error.

JM:

An example is the assertion, echoed by almost all AAT versions, that paleoanthropologists claim that early hominids lived on "the arid savannah", which AATers typically -- and inaccurately -- characterize as a waterless, treeless plain. The AAT is then said to be the only sensible theory, since early hominids were probably not well adapted for surviving on a treeless, waterless arid grassland. But paleoanthropologists don't make this claim; they actually refer -- quite clearly -- to an early hominid habitat consisting of mixed vegetation: trees, bushes, and grass.

Although online Elaine Morgan has derided this correct version as some new idea -- a sort of last ditch desperate act by weaselly scientists to try to keep hominid evolution on land -- this is the definition of savannah which has been used for about a century.

EM:

But whenever it was used to explain a distinctive human feature it was usually attributed to the cruel heat of the overhead sun. Only when they were out on the grass they were evolving?

Your statement above is yet another strawman version of what paleoanthropologists say; you seem incapable of even seeing the word savannah without building your strawman. It *is* a complete reflex action for you now, isn't it?

JM:

The AAT theorists have also gone for that classic technique among marginal theories, the shifting target. In explaining (excusing?) this shifting target strategy, AATers simply say their theory is adjusting to new evidence, as any scientific theory should. However, actions speak louder than words, and when we examine AATers' actions,

EM:

Dear Jim, All we have to go on are one another's words..

For a writer, words are action, Elaine. Haven't you yet picked up on that in nearly 50 years of professional writing?

JM:

As one example, with the change in the AAT mentioned above (i.e., a less aquatic wading ape), the AAT claim that these hominids would not and could not evolve bipedal posture and locomotion without being in neck-deep water falls by the wayside... and yet they still use that claim.

EM:

I never said neck deep. Alister never said neck deep. Marc never said neck deep. Could we have a reference please? With page-numbers?

As just one example, you said it on page 47 of *The Scars of Evolution*. This is only one of the many times you and others -- including Hardy, as reprinted in your 1982 book, *The Aquatic Ape* (pages 140 and 143) -- have stated that these proto-hominids were in up to their necks, sometimes even using the phrase "head-out immersion"; the AAT/H accounts also have typically stated that wading was necessary for an early hominid to maintain upright posture, which wouldn't be the case unless they were in way over their waists. In the case of a shallow-water wading animal, this just doesn't work, yet it's still done. This deep-water wading has also always been the necessary crux of the AAT/H's explanation of why we have hair on the tops of our heads (they ignore the large amounts of body hair many modern humans have).

JM:

When scientists have facts pointed out to them which prove their line of evidence is wrong, they drop that line of evidence. AAT proponents rarely do, and even when they do, others continue to use it. Sometimes the very same person who says they've dropped it continues to use it (example below).

From: Elaine Morgan in sci.anthropology.paleo on 13 Jul 1995: "In *The Scars of Evolution* you will seek in vain for a mention of hair tracts or swimming babies or the direction of the nostrils. New evidence for AAT comes in faster than anything I have to keep in abeyance pending further facts."

Note: All these things, however, were stated in 1982's *The Aquatic Ape* (although Morgan had also falsely claimed her inaccurate statements about the evolution of the nose only appeared in her 1972 book).

EM:

My suggestion about the function of the "roof" over the nose was not a statement of fact but a hypothesis.

"Hypothesis" does not mean "wild guess with no supporting evidence whatsoever", which is the only accurate description of your nose and nostrils claim.

JM:

From: Elaine Morgan in sci.anthropology.paleo on 19 Jun 1995: "The nose - yes, I did once make the mistaken assumption that the nasal spine was in evidence prior to H. erectus."

EM:

I was not retracting the hypothesis. I was saying that I was mistaken in thinking the nasal spine predated H.Erectus., It does not necessarily mean that the nostrils themselves did not already point downwards at that date.

So you're saying that you were only dropping your nasal spine claim (which was well-known to be untrue for several decades before you first made it), and that the rest of your statement at that time was simply a piece of trivia that wasn't meant to be a response to the posts you were responding to? Odd, to say the least; and the context of your post then clearly tells a different story.

JM:

We just don't know, if this evidence has been dropped, why do I bother documenting its inaccuracy here?

There are several reasons:

Although some of these ideas are dropped by one AATer, other AATers continue to use them. For instance, while Elaine Morgan says she dropped the "swimming babies evidence" after her 1982 book, Marc Verhaegen has continued to use it (without noting that it is a response common to all infant terrestrial mammals tested).

Even though an AATer may say they've dropped an idea, the same person may actually continue to use it. For instance, in the sci.anthropology.paleo newsgroup post quoted above (from 13 July 95) Elaine Morgan says she dropped the "evidence" about the direction of the nostrils after her 1982 book, but she subsequently began using it again online -- starting 14 July 95: the following day(!).

EM:

I didn't say I had dropped the swimming babies evidence. I said I did not deal with it in "Scars."

So it was another of those non-responsive trivia tidbits again? I see. But it was bogus the first time it was mentioned; the evidence that it was bogus was well known since the study and paper on it in the late 1930's. The only way to "deal with it" in the first place would be to either not use it or admit that it provides no support at all for your hypothesis. My apologies for saying that you had at least at one time come to your senses and accepted the facts which had been known, at that time, for some 45 years. I accept your present statement that you had never accepted this reality at all.

By the way, the first part of what they've quoted me as saying up there ("We just don't know,") not only isn't mine, it didn't come from any part of my site. Why they added that is beyond me, but let me say again that when quoting, you're not supposed to just make up words and put them in others' mouths. But then, Morgan *has* won awards for writing TV docudramas, which specialize in doing just that.

JM:

She's also using that info, as well as the salt, sweat, and tears info she said she'd dropped, in her latest book.

EM:

"Using" the salt sweat and tears info? You mean "Using it" to point out that my original hypothesis didn't hold up and why? How very obstinate of me. When all you wanted me to say was "I lied in my teeth with the object of misleading everybody especially Jim Moore." Have you actually read that last book? I hope your readers will.

What you are primarily using it for now is to claim that you are a competent, honest researcher who was simply misled by the info available at that time. However, at the time you originally made your tears, sweat, and salt claims, the evidence that showed it to be false was available. In fact, it was in the very sources you used as "support". So you held in your hands the evidence that showed your views were wildly in error, yet you made them anyway. Now you attempt to mislead people as

to the gross seriousness of your action, since it can't really be said to be an error: it was either gross incompetence or deliberate dishonesty.

JM:

Relevant Questions for the Aquatic Ape Theory

Convergence:

These are five real aquatic traits, traits shared by virtually all aquatic mammals, even those which are not closely related. Why don't humans exhibit these actual aquatic adaptations?

EM:

You're a bit out of date on this. It is now called "Homoplasy" which is perhaps why you couldn't find much recent gen on it.

Answer to all five:

Because (a) they are primates. Evolution (and homoplasy) has to start with where you come from, not where you would like to go. And (b) we were not in the water nearly as long as most of the aquatics. Think sea otter, not dolphin.

The sea otter *is* one of the aquatic mammals with the traits my list refers to. These aquatic traits, as I clearly pointed out, vary between those creatures and their terrestrial cousins. In none of these ubiquitous aquatic features do humans vary from our cousins. In addition, the AAT/H claims changes that are far more radical than any of these; why was there not time for these actual ubiquitous aquatic traits when there was time for more radical changes?

I'm familiar with the term homoplasy; it's hardly new ("The term 'homoplasy', introduced by Lankester in 1870..." Alec L. Panchen, ["Homology-history of a concept"](#)). Homoplasy is simply a category which includes parallel evolution and convergent evolution. Convergent evolution is what produces these actual aquatic traits, just as it is purported to produce the not-actual aquatic traits the AAT/H refers to.

JM:

Humans have young that are born less developed than our relatives, and they develop more slowly as well.

EM:

This process set in much later. Look it up.

We actually don't know for sure when this happened, although it's likely it was part of the suite of changes seen during the transition from australopithecines to homo. We do know that the marine mammal trait is both ubiquitous and radically different from their terrestrial relatives and from us and our primate relatives.

JM:

All marine mammals produce milk that is extremely rich in fat and protein, and very low in lactose (milk sugar). It ranges from a low of 20-25% fat for sea otters to 30-60% for pinnipeds and whales; protein ranges from 5-15% or more; lactose is virtually non-existent. By contrast, human milk and cow's milk are about 2-4% fat and 1-3% protein; lactose in the milk of terrestrial mammals is typically 3-5%. The lactose content of human milk is as high as 6-8 percent. Why are humans so unlike all marine mammals and so like terrestrial mammals?

EM:

Some of those seals feed their young for a matter of weeks only, in a very cold climate. We fed ours for years in a very hot one.

"Some of those seals" do indeed, while many others "of those seals" suckle their young for very long periods, even years, in hot climates, yet they *all* have that trait. And it's not just seals, but all marine mammals: seals, whales, manatees...and yes, sea otters too. All of them. We, on the other hand, are radically different, and instead are very like those mammals' terrestrial relatives in the composition of our milk. The pattern of these radical differences indicates that this is not due to relatedness but is instead a convergent trait (you may say "homoplasy" if you like) due to adaptation to either a marine environment (for those with milk high in fat and protein, and extremely low in lactose, unlike us) or a terrestrial environment (for those with milk low in fat and protein, and much higher in lactose, like us).

JM:

One AAT claim is that we evolved in saltwater and therefore adapted in the same manner as aquatic animals, with convergent evolution supposedly evolving a salt excretion system like that seen in sea birds and crocodiles (and many terrestrial reptiles and birds).

EM:

Claim "is?" Present tense?

Would you oblige me with a signed statement to the effect that "I Jim Moore have read the Aquatic Ape Hypothesis? and not just caught sight of the book jacket in a shop window?" If so you could have fooled me.

There it is again, that incensed tone as you make the assumption that I must always be referring to you when I mention AAT/H claims. Didn't you just wonder where I got the idea you did that?

We've already seen your propensity to begin using again lines of evidence you say you've dropped (sorry: kept "in abeyance pending further facts"); there is also the fact that your previous work, both your books, your articles, and things like your leaflet, continue to be seen and therefore misinform people anew, who, unaware they have been misled, parrot your words. I am countering those continuing claims.

And if I really did do that detailed critique of your book, reciting chapter and verse, after just catching sight of the book jacket in a shop window (shop window? who's dreaming now?), I think James Randi owes me some money!

Forgive me, I am tired of this. I've just fast-forwarded and see it goes on for much longer than I can stay awake, I am, as Jim vividly points out, a very aged wrinkly decrepit old soul with a Fred Flintstone tablet to carve out my answers on. The chisel is getting too heavy for my bony crooked fingers.

I have *always* intimated both that you used a computer and were *far* from decrepit, and I have treated with you in that spirit; it has been your *supporters* who've said you post to the net with a manual typewriter and that you're too far gone to handle the correction of your many mistakes.

Only one thing never changes. Jim supplies a million reasons why what I think is wrong. He never has and never will stick his neck out to tell us how he would explain a blind thing about human evolution. With a quarter of the time and work he has put in he could surely have come up with a theory that would knock Hardy's into a cocked hat. What a waste of years and intelligence.

There are many people writing about human evolution, providing a lot of fascinating and accurate theory-building. I am simply taking one theory, and pointing out its abundance of errors. I can certainly understand why you would prefer I didn't, but my correcting the errors introduced by the theory's authors hardly seems a waste -- indeed, this is an essential part of science. It's the keystone on which science builds. Spending 30 plus years spouting the same disproven false "facts", on the other hand, does seem a waste, but only if one thinks the theorizer was capable of more than that.

p.s. I would have thought a million reasons was enough to sink in. Seems not.

Notes on BBC Radio 4 program

"Scars of Evolution" by David Attenborough

2-parter broadcast 12 and 19 April 2005

Notes on the first half hour of David Attenborough's "Scars of Evolution" radio show on BBC Radio 4, broadcast Tuesday, 12 April 2005

Attenborough starts off his introduction by talking about long distance swimmers, whose feats are certainly remarkable, but he does somehow neglect to point out that these are highly trained athletes who perform feats that ordinary humans do not, and cannot, do.

He then segues into running, leaving off any talk of athletes this time, but mentions that "our top speed is somewhat less than a rabbit" but doesn't provide any equivalent comparison for swimming speed; he just says "but we can swim". We certainly can, but why dodge the comparison for swimming speed when it was apparently so important for running speed?

The answer is, of course, because our swimming speed -- even for the top athletes over a very short distance -- is abysmally slow. While the best runners can do a mile at about 15 1/2 miles per hour, the very best Olympic swimmers can't even manage to do 6 miles per hour for 3% of that distance. This is pitifully slower than most swimming animals, and it's frankly not all that impressive that the fastest athletes in the world, in the shortest sprint event, can nearly be matched by a fast walker -- a little over 5 mph. To make this difference clear, when I was in my twenties I used to walk 20 miles in 4 hours, and I was not an athlete. So I could walk for 20 miles at nearly the speed that the very best Olympic swimmers can only manage for a very short distance.

Attenborough continues the swimming theme with "and dive underwater, to considerable depths, holding our breath for upwards of [through ?] 6, 7, 8 minutes". Again, these are very highly trained people, not anywhere near the norm. Why not use the norm? Perhaps because he'd be forced to acknowledge that untrained humans can be outdone in breathholding by untrained dogs, and that doesn't sound all that impressive either.

Having set the stage, Attenborough goes on to sound bites from a select few speakers. In doing so he sets up several false dichotomies and begins setting up an impressive-looking strawman. In fact, much of this first half hour is spent doing that, with the rest being portraying Morgan and Hardy et al. as victims of a blind, irrational, scientific establishment intent on protecting their territory rather than finding out stuff. This is something you see done a lot in accounts of the AAT/H, and especially when Morgan is involved. It plays well.

He begins in what seems an innocuous manner, with senior anthropologist Philip Tobias saying "Let's just talk about water and human evolution". That's mild, and perfectly reasonable, but the cute little trick here is that this implies that paleoanthropology has not talked about "water and human evolution". Does Tobias not know this, or was he just edited to suit? I don't know, but judging from other statements Tobias has made (and makes later in this program) I suspect he just doesn't know. This may sound odd, but anthropology is very fragmented and Tobias, as a "bones and stones guy", doesn't necessarily keep up with the rest of anthro theory. He should before spouting off on it, of course, but not everyone wants to do that work on top on their own specialty -- it isn't easy, after all.

We're at about the 6:00 minute mark now, as anthropologist Leslie Aiello is heard saying "what Dart could be said to have done is to establish the savanna hypothesis". The idea of the "savanna hypothesis" is important in discussions of the AAT/H because it's the method by which AAT/H proponents inaccurately claim that paleoanthropologists use the same sort of environmental determinism that they use.

Attenborough then goes on for several minutes about "Ardrey and others", "Naked ape", "Killer ape", "Mighty hunters", etc., laying the groundwork for making the claim that "even 40 years later it's still the basic account of early humans most commonly told in schools it gradually became apparent through the 80s and 90s that the story was a bit oversimplified. We didn't head out from the forest and make a completely new life on the open plains, and we didn't make a simple switch from grazing on fruit to chasing down big game and eating their meat."

Kind of a shame that he completely ignored the work of the influential women in anthropology who changed this way of thinking. Starting with Thelma Rowell, and to some extent Jane Goodall, in the mid-1960s, and with an able assist from Richard Lee (who's male but has that "annoying" habit of doing good accurate work regarding male and female roles in society), and on through Sally Linton (nee Slocum) in the late sixties, then on through Nancy Tanner, Adrienne Zihlman in the 1970s, these ideas about males and hunting being at the root of hominid change were changed. But note that these were ideas about *behavior and food getting and social interaction*, not about environment per se.

But even then the talk was of a mosaic savanna woodland -- although many people think of savanna as meaning a treeless plain, and Morgan and other AAT/H proponents have encouraged this belief, anthropologists know differently. Check out my short page on the [definition of savanna](#) for clarification.

Attenborough either doesn't know this or doesn't want to mention it, and if he got his cue from Morgan (as seems likely) this is not surprising, because Morgan has consistently ignored these contributions by women. (For a professed feminist this is really, well frankly, disgusting.) Well, now Attenborough works it a bit to try to portray Tobias as being at the forefront of looking at environment (ignoring more women, by the way, like Elizabeth Vrba and A. Kay Behrensmeyer) and changes history by ignoring the work they did in the 1970s and letting his (Attenborough's) statement that "but by 1995 the doubts were overwhelming" make it seem that the mosaic of woodland, forest, and mixed savanna was new info then. This allows him to portray Tobias heroically proclaiming claiming that "the savanna hypothesis is no more -- open that window and throw it out!" At which point those in the paleoanthropological community who had been paying attention for the past quarter century looked at each other and said "the WHAT?"

Attenborough then plays a clip of Tobias saying "of course if savanna is eliminated as a primary cause or selective advantage of going on two legs then we are back to square one". This reveals two basic, and rather sad, problems -- it demonstrates that Tobias incorrectly thinks that paleoanthropological theories were environmentally deterministic and it shows that he just wasn't familiar with the work done in that area for some 25 years. Of course it wasn't his specialty -- but

you'd think he'd have paid a little attention to it. You see the actual theories of human evolution, being based on things like food-getting and social interaction rather than being environmentally deterministic, don't have to "start at square one" every time new information about habitat comes around.

Attenborough, however, buys the "square one" claim, but says that luckily, there's an alternative: the AAT/H.

This idea, he says, explains various human characteristics, for instance: "our bipedal locomotion on two legs" (as opposed to some other kind of bipedal locomotion, I suppose), "strange hairlessness", "layer of fat just beneath the skin", "big brain and unique language skills", and "the curious paradox that noses are very large but our sense of smell is comparatively poor".

With nearly 20 minutes left in the broadcast surely we'll hear the evidence for these claims. For now we have to be content with some more building of the myth of the close-minded establishment -- he even uses the word "establishment" to make sure we get the picture: "but in establishment circles the alternative explanation was regarded as too bizarre and too radically different from the accepted savanna story to be taken seriously." "But why was it regarded as bizarre pseudoscience and why did mainstream hostility continue for nearly forty years?"

Now we have the formal introduction of Elaine Morgan, who Attenborough says in 1967 was "a young woman from the Welsh valleys" (10 or 20 years ago I would've probably quibbled about a 38-year old being described as "young" but that's what getting older does to you. :) And through her, Hardy's original idea, which was initially inspired by noticing the "peculiar layer of fat tightly bonded to the skin that humans have and other primates lack". Never mind that the foremost researcher on the evolutionary significance of fat, Caroline Pond, has noted that this layer is also seen in any primate which essentially overeats as humans have been able to do for so long. Never mind, of course, because noting that fact makes it seem less "peculiar" and more "of course".

According to Morgan, after Hardy gave his talk at the sub-aqua club, prominent anthropologist Le Gros Clarke called Hardy and "Alister, never do that again". (According to some, Le Gros Clarke then went on to alert the entire worldwide anthro community to stonewall poor Alister and his idea, and of course they all clicked their heels together and said "javal!" -- Morgan and Attenborough, to their credit, do not indulge in this fairy tale image of a cohesive anthro conspiracy.)

Not that Attenborough doesn't indulge in some storytelling. It's part of his ongoing attempt in this section of the broadcast to develop the idea of the blind, irrational, scientific establishment. Morgan's claim that Hardy's idea was not accepted because

of his not being an insider comes next. "He was an outsider to the extent that his field was marine biology and not anthropology so they held that against him as well." Attenborough then invokes the spectre of censorship (by the great anthro conspiracy, apparently): "One might think it odd that an eminent marine biologist and fellow of the Royal Society would be barred as an outsider from venturing some observations about human evolution..." Apparently not having your poorly thought out and supported idea immediately accepted is equivalent to being "barred".

This is his intro to painting Morgan as even more of an outsider, as he continues "... but academic boundaries can be fiercely patrolled, and if Hardy was regarded as beyond the pale what of Elaine Morgan." Ooh, just think what the GAC (Great Anthro Conspiracy) would do to her... The humanity!

So for Attenborough's account, the anthropologists' lack of enthusiasm (which apparently is equivalent to "barring" someone from "venturing some observations") certainly couldn't be because Hardy's idea didn't make sense and used bogus "facts" -- no, of course not.

Attenborough then does some more positioning of Morgan as an outsider so he can say that "professional anthropologists liked the idea of the aquatic ape theory even less coming from Elaine Morgan than they had when it came from Sir Alister Hardy". Well, it certainly was heard more; was it actually liked less? No matter to Attenborough's account, actually; he's not really interested in the reaction except as a way to help in his building of the myth of the closed-ranks to the outsider assailing the ivory tower etc... which is, by the way, a staple of Morgan's writing.

Attenborough again: "It didn't matter that she had researched the subject in great depth" and "if anything, the accumulation of further detail that she had painstakingly carried out made the anthropologists dig their heels in all the more firmly". Not, of course, because the idea didn't make sense and used bogus "facts" and misunderstandings of evolutionary theory, and misquoted people, and on and on -- no, no, no, of course not.

Morgan says of her first book, "the scientific reaction was bitter anger and contempt" Maybe from where she sat; what I saw was more in the category of "amused", but of course that would be an annoying reaction to a person who's claiming to have a grand new theory that will overthrow the whole of a branch of science.

Attenborough now brings in some others to bolster his idea that Morgan and the AAT/H are beleaguered outsiders beset by irrational close-minded anthropologists. In fact, Daniel Dennett actually refers to the "irrational hostility she engenders", which leads me to a question: since she uses false facts and misunderstandings of

evolutionary theory to support her hypothesis, is it irrational to oppose it, or is it sensible to do so?

Dennett that says there hasn't been "the right sort of measured and sober response; at least if there has been I haven't been able to find it". Well, I'd have to say that Dennett didn't look too hard -- try a Google search, Dan :) But then maybe my site, or Adrienne Zihlman's articles, or the articles in the book *The Aquatic Ape: Fact or Fiction?*, aren't "the right sort". Oh well, we can't all have the right stuff.

Attenborough's back with more myth-building, suggesting that it's just because they were outsiders the idea provoked not just caution but "something more visceral, even jealousy". That's it, they were all just jealous. Look, I know many people, most people actually, haven't been inside academia or near enough to see the workings, but this is a ludicrous idea of how it all works. Frankly, if people like an idea they just grab onto it and say a bit more about it, and then they're in the club too. Happens all the time; in fact, you see it with the reaction to my late wife's work, where the stuff she and Zihlman did was adopted pretty much whole hog by a couple of guys, Glynn Isaac and later Owen Lovejoy, who added some (not too well-founded) additions, and got a lot of mileage out of it. If people thought that Morgan and Hardy made sense they would've grabbed onto it so fast it'd have made Morgan's head spin like a top.

But to Attenborough the only possible explanation can be irrationality and jealousy, not of course because the idea didn't make sense and used bogus "facts" and misunderstandings of evolutionary theory, and misquoted people, and on and on -- no, no, no, a thousand times no, of course not. He brings Graham Richards on to help him out; he agrees it was pretty much just jealousy, or maybe that "if it was true one of us would have come up with it first" -- the scary thing is that when people think others think like that, it's often because they themselves think like that -- and that anthropologists said dismissively "Oh she's cobbled together a kind of collage of different facts and figures". Richards points out that a "key thing of a good scientific theory that it does this linking job on a lot of phenomena that were hitherto thought to be totally unrelated", but somehow neglects the equally important part, that those links and phenomena and facts and figures really need to be accurate instead of false.

Attenborough goes on to say that in her other 4 books she started "providing notes, referencing every citation" which is very nearly true for her last book on the subject, and she did better on her 1990 book. Before that she really didn't, and you can read my overview of her 1997 book to see how many errors and false claims she did even in that, arguably her best referenced and supported book. Still, better than nothing.

Uh oh, it's 25 minutes in and time for the Wegener comparison. I have a page on [Wegener](#) and the way he's used by Morgan (and many others) as a comparison, and I explain how she, like others, describes his work and the reaction to it inaccurately. This all shows up here again, with a bit of an extra touch. The standard, and inaccurate, part is when Attenborough describes Wegener as a meteorologist who was therefore the "classic outsider" (he was trained initially in astronomy and did work in meteorology and is generally referred to as a geophysicist) and says he was "roundly ridiculed and rejected by geologists for over 30 years" until "Wegener's plate tectonics became the accepted paradigm". Well, as I explain on my Wegener page, he was supported by many and opposed by others, rather than "roundly ridiculed", but more importantly, it wasn't "Wegener's plate tectonics". Plate tectonics is a mechanism, and that was the whole problem with Wegener's idea -- he had no mechanism that came remotely close to explaining how the continental movement he proposed could actually happen. It wasn't until the 1950s and new equipment that could examine the ocean floors that a mechanism -- plate tectonics -- was found. Plate tectonics was not Wegener's by any stretch of the imagination -- continental drift was -- plate tectonics wasn't. This is an important distinction because here Attenborough is using a rhetorical trick, either because he's ignorant or he's deliberately trying to make a connection in people's minds to make Wegener's opponents seem, well, irrational, just as he's trying to make Hardy and Morgan's opponents seem irrational. He does this by ignoring the fact that Wegener's idea of continental drift didn't make scientific sense without a mechanism and that Wegener had nothing whatever to do with the mechanism (plate tectonics) that was eventually discovered.

And as I stated on my Wegener page, the theoretical difference between the AAT/H and more conventional theories of human evolution isn't in what happened (hominids evolving from an hominoid ancestor), but in the *mechanism* for it. The mechanism (an aquatic past) is the different special thing the AAT/H brings to the table. and, just like the mechanism in Wegener's theory, it falls flat.

Then Attenborough makes the connection explicit as he switches to describing the supposed reaction to the AAT/H, using phrases like: closed ranks, ignored it, denounced it as bizarre, there's no testable evidence, and the ultimate "no equivalent to the spreading sea floor". Poor Morgan is Wegener, you see, all who oppose her are blind supplicants to the ivory tower. But, you see, when the "evidence" used to support it is largely false, some people may consider this important. Not Attenborough, apparently, but some people.

"Missing evidence is finally appearing" next episode apparently; about time; over 26 minutes into the half hour and you couldn't come up with any yet?

Ah well, we're told "meanwhile the mainstream anthropologists' view of where humans really grew up appears to be moving, hesitantly, down to the seashores". We've always known that somewhere along the line, some water resources were used, just as we moved into many different environments; the only people who dispute that this was widely accepted are AAT/H proponents. This, however, is not the AAT/H.

He does have Leslie Aiello saying a very important thing here, which he cuts off with an edit, "it depends what you mean by semi-aquatic; do you mean one toe in the water or..." .[and she's cut off]. This now is one of the longest running and biggest flaws in every version of the AAT/H; no one wants to explain what "aquatic" or "semi-aquatic" mean. And when do they try they get very vague, unlike when scientists do the same thing, and then they have trouble explaining how we evolved features they say are similar to certain, usually vaguely referred to aquatic creatures (who turn out to be whales, seals, and sirenia) when we supposedly were just strolling along shorelines and such.

The first half hour closes with Elaine Morgan herself comparing her and Hardy's plight to Wegener.

Notes on the second half hour of David Attenborough's "Scars of Evolution" radio show on BBC Radio 4, broadcast Tuesday, 19 April 2005

Okay, this is good. The first half hour had, really, no evidence whatever -- just claims of victimization of poor Hardy and Morgan -- which by the way is one of the classic logical fallacies: the "Appeal to Pity" or "Argumentum ad Misericordiam", which Stephen's Guide to the Logical Fallacies (link on my links page) describes thus: "the reader is persuaded to agree by sympathy". But now we'll get some evidence mentioned -- after all, we have 30 minutes of uninterrupted radio show ahead.

Attenborough's narration mentions some of these features: "The hypothesis claims that the features that distinguish humans from the other apes, standing upright, naked and sweaty, swimming and diving, with our very fat babies, big brain and language skills, are best explained as water adaptations."

But I found after listening that I was foolish to take Attenborough's word that we'd hear any evidence at all for most of these claims (and the claims of the first half hour) because as it turns out we only hear about those last three. And even on those we have dubious and misleading research offered as evidence. In fact,

besides the "Appeal to Pity" logical fallacy mentioned already, this half hour is chock full of logical fallacies, so much so that it would make a good class exercise on the subject. I'll mention some others as we go along.

First we have some hyperbole from Attenborough which, in a way, I regret having to mention.

"For forty or more years the accepted crucible of humanity was the open savanna grassland of Africa and the aquatic alternative was ridiculed. Then, really since the early 90s the consensus began to fall apart. Why? What new evidence has come to the fore in the last ten or so years to cause such a paradigm shift to drive the anthropological mainstream off the savanna and, with a certain amount of pushing and shoving, and cautious hesitation, down to the water's edge?"

First a warning: whenever you hear the words "paradigm shift" brace yourself for BS. You won't always be hit with it -- sometimes the words really can be used and be true -- but the world of fringe science, and of pseudoscience, is where you hear it used most. In this case, there's something more; that last half sentence unfortunately reminds me so much of the continued claims by the "intelligent design" movement that scientists are dropping evolution -- it doesn't please me to point out when AAT/H proponents talk like, or use the tactics of, the creationist and ID movements, and I wish they wouldn't do it anymore. Until they stop doing it, though, I'll have to keep pointing it out when I see it.

And on the logical fallacy front, note that Attenborough has already used the Strawman Fallacy when he talks about the "savanna grassland" instead of mixed woodland or savanna mosaic as it's called.

Two minutes in and Dr. Michael Crawford is brought on to talk about the brain.

Crawford tells us that on the savanna "Every single species without exception lost its relative brain size as it evolved bigger." Which isn't true but we'll get back to that. Then Attenborough tells us that "Crawford and his team" (sometimes I wish I had a team) found that "First that the brains of all other savanna animals had shrunk in relative size as they evolved larger bodies. by contrast the human brain had grown 3-fold." Which is both a strawman and also isn't true but we'll get back to that as well. Then Crawford again: "When one compares the land-based with this universal collapse of brain size, with what goes on in the marine system I mean you've got the dolphin which is about the same body weight as a zebra; the zebra has about 360-70 g of brain in its head; the dolphin's got 1.8 kg."

Yikes.

This is what happens when you have people who

- A) have no background in the study of evolution, and
- B) don't do their homework on it before they start
- C) making statements on subjects they know little or nothing about

Or, possibly, he does know what's wrong with what he's saying and is deliberately misleading others about it, which would actually be worse.

Here's the most basic problem with his statement -- he's comparing apples with oranges, or in this case, predators with herbivores, and he's picking his examples rather carefully. One might say suspiciously carefully. He's also ignoring the fact that all higher primates have relatively large brains, just as the various dolphin species do. So he's essentially expressing amazement that humans are like their close relatives instead of like zebras. I mean, really. I've had to point out before the silliness of Morgan's amazement that we are like our relatives instead of like "the wild ass and the camel"; does it really have to be pointed out *yet again* that it should not be considered odd that humans more closely resemble their primate relatives than they do extremely distantly related ungulates?

Brains typically do not increase in size (as measured by weight) as much as bodies do. This is not unusual; it's something found in many aspects of scaling, such as bone diameter as one of many examples -- an animal which is twice as large typically does not have bones which are twice as large in diameter -- but these differences in scaling are broadly predictable. The study of these scaling relationships is called "allometry" -- and brain size is one of many features studied as part of allometry. In other words, large animals tend to have larger brains than smaller animals, so how do you know whether the larger brain is actually any bigger than you would expect it to be if it were merely due to a larger body size? There's a way you can compare the expected brain sizes of different mammals of widely varying sizes, and see whether they are average, small-brained, or large-brained.

In dealing with increase of brain size vis a vis increase of body size there's a ratio which holds generally true across all types of mammals -- it's .7:1, (brain size to body size; sometimes said to be .67 or .68 instead of .7) and those mammals which are at exactly this ratio are said to have a brain EQ ("encephalization quotient") of 1. This ratio actually holds true in other animals too, but you can't compare, say, reptiles to mammals with it -- reptiles with other reptiles and mammals with other mammals is fine, except for really huge mammals, like sperm whales or blue whales for instance. And it doesn't tell you how a brain is organized or provide an exact measure of relative intelligence, although it does offer a pretty good rough guide for that. In other words, because one animal is a 1 and another is a 1.1, that's not a huge difference and might not mean anything much, but if you have a 1 and a 3, okay, you've got something, and even smaller differences do tell us

something. You have to be careful about comparing EQs, especially when they're from different sources; differences in calculation, and of course, the variance in a given animal's body weight can affect the EQ -- obviously if you put on 30 pounds, your EQ goes down, but you didn't actually get dumber (probably not anyway :). But it does provide a good guide -- as good a guide as we're likely to get.

Among the things which are well-established from these studies, we know that predator species generally have higher EQs than prey species, prey species which use active predator-avoidance strategies generally have higher EQs than prey species which use passive predator-avoidance strategies, and animals which live socially, especially in relatively small groups with a lot of individual interaction, tend to have higher EQs than other species.

What you're seeing in that fact is that, due to selection pressure during evolution, the more your species *needs* to think, the higher their EQ. Larger brains are not necessarily better, because the larger the brain, the more energy it takes to develop it and feed it. So animals seem to develop the size brain they need and that's about it.

Think of it this way. Remember that trip you took where you had the huge suitcase and that other guy you were with had a small suitcase? You figured you'd have everything you needed and then some and you did; the other guy had just barely what he needed and no more. Remember how much more rested he was at the end of each day, and how much less energy it took for him to do everything? Just how useful was that huge suitcase that had more than you needed? Not very, right?

Same thing with brains.

Notice that when Crawford carefully picked his examples, he chose an aquatic species which fits all those higher EQ criteria and compared it to an herbivorous species which fits into pretty well all the lower EQ categories. Then he pretends we should be surprised by the result.

Predators (which includes dolphins) typically have higher EQs than herbivores, so instead of making his apples to oranges comparison, why didn't he compare the herbivorous zebra to an aquatic herbivore, like a manatee or a dugong? Is it because manatees and dugongs are on the lower end of the scale for mammals' EQ, doing somewhat poorer than zebras? Or just because he's talking about diet? If he wanted to investigate the difference in diet as the cause (which seems to be his actual aim) why didn't he pick an animals which have a diet at least somewhat like hominids, instead of a carnivore versus a grasseater?

Instead he picked the absolute highest EQ aquatic mammal (and a predator) and compared it to an herbivorous hoofed mammal, which typically have low EQs no matter where they live, and then pretended this difference was somehow odd or explainable only by an aquatic diet.

What if he had compared a seal, which after all eats a lot of fish, to a savanna baboon, which does not? Depending on the type of seal, he'd have had either about the same EQ or slightly to much smaller EQ for the fish-eating species. Well, that obviously wouldn't do for Crawford's thesis, so he ignores this uncomfortable fact. This another of the logical fallacies, this one Stephen's Guide refers to as the Fallacy of Exclusion: *evidence which would change the outcome of an inductive argument is excluded from consideration*.

He also, of course, uses the strawman of the grasslands savanna (instead of the woodland savanna mosaic that's been used for at least a quarter century) in his zebra example. But even when using this strawman he isn't accurate. For instance, savanna baboons and savanna-woodland vervets have rather larger EQs than most non-ape African forest primates, which also contradicts Crawford's thesis that savannas can't support the increase of brain size. So does he mention them? No, obviously he didn't.

In fact he ignores anything that might call his claim into question. More "Fallacy of Exclusion".

So the end result is that Crawford is implying that this is odd and needs explanation, when actually what Crawford and his team have "discovered" is exactly what you expect. More on EQ and what you can do, *but shouldn't do*, with it [here](#).

There's the mention of Omega-3 fats and iodine as requirements for normal brain development -- that's fine. But then there's a major non sequitur, which is yet another of the classic logical fallacies, when Crawford says "the only way homo sapiens could end up today with 2% of his body mass as brain would be to find an ecological resource that would provide him with the nutrients that are require for the brain to grow and the only place you can do that is at the seashore." It is *not* the only place; there are people who've calculated the amounts of energy and DHA and/or LNA fatty acids needed to develop normal brains and it's available even in savanna conditions. He also mentions iodine and that's available too. So here he manages to use two logical fallacies at once: the non sequitur and the Fallacy of Exclusion.

One of the odd things that pops up in the idea of the supposed need for an aquatic diet to get larger brains is the same fallacy that people use when they take megadoses of vitamins -- too little isn't good, enough is good, so lots more is

better. So instead of looking at dietary factors in terms of whether there's enough in a given environment, they look at where there's lots, and compound the error by only looking at a couple elements of a diet -- DHA, and here, iodine. So you wind up with errors like Crawford's above and sometimes *really* silly statements like Attenborough's next one.

This is the statement, which may sound valid to an unschooled audience (it's supposed to) but which to someone with any knowledge of evolutionary theory (or nutrition, for that matter) should sound foolish:

Attenborough: "Some researchers were still skeptical. they suggested that the brains and the bone marrow of big game on the savanna might supply the essential DHA. But if that were the case, then surely the dominant predators, the big cats, far better equipped than a small fruit eating hominid ape to crunch through bones and skulls, would have got into the brain and marrow first, and thereby grown their own big brains. Well, they hadn't."

Frankly, this is so uninformed it sounds like a parody of creationist thinking. First, big cats, being predators, do have larger than average brains for their size. Second, and more apropos, larger brains are not necessarily better, and the larger the brain, the more energy it takes to develop it and feed it. Remember the suitcase analogy?

Another way you can see the error in this line of thinking is by asking why all creatures which eat fish don't have huge brains. Diving ducks, sea gulls, herons, otters, seals -- why isn't Attenborough asking why they didn't outbrain us? Why aren't they at least dolphin smart? Mind you, they shouldn't necessarily be, not if you realize that large brains are not always an advantage, as all sensible evolutionary theorists do. But if you're not sensible and imagine, as Attenborough (taking his cue from AAT/H proponents) does, that diet trumps all and therefore that big cats should be exceptionally brainy due to their diet alone, then why doesn't he wonder about all those not overly bright fish-eating species? This dietary determinism is an offshoot of the environmental determinism seen in the AAT/H, and makes as little sense.

Crawford: "Believe you me, if you want to try and get the brain out of a buffalo skull, you're going to need a hacksaw to do it. It's no joke. And it would certainly would not gone round a tribe for argument's sake. And by the time you got it home to your, where the women were, it would probably have festered and be foul."

One fairly small thing here that annoyed me; when Crawford said about obtaining and eating meat "by the time you got it home to where the women were..." This assumes a modern human gatherer-hunter lifestyle with home bases; that kind of

thinking went out at least a quarter of a century ago, although admittedly some folks still like the style.

More important here is that even if we were talking about taking back food to a home base where the women sit a'waiting, Crawford's complaint is likely wrong -- modern hunters take meat back plenty of times and plenty far; and actually one of the reasons that some researchers have suggested marrow and brains is that these bits last somewhat longer since they're essentially packaged up in containers -- especially marrow; it stays fresh enough for days. Not as fresh as the way we'd want to eat it, mind you, but plenty safe enough for our less discriminating ancestors.

I also find it suspicious that of all the examples of braincases Crawford had at hand, he just happened to pick an animal that

A) is considered even by modern hunters to be one of the most dangerous animals in Africa

B) is therefore highly unlikely to have been hunted especially by very early hominid hunters, and

C) is partly considered so dangerous precisely because it has an unusually hard skull coupled with thick horns which make a head shot with a powerful rifle an iffy procedure.

Why not pick any of dozens, or even hundreds, of far more likely prey species? Odd that he just happened to mention that one, and only that one, out of (at least) dozens.

Then a bit later, when Stephen Cunnane compares human babies' fat to baby chimps and gorillas and found it different -- why not compare to all mammals; it's unique. Of course comparing it to those other mammals (including aquatic mammals) wouldn't advance the AAT/H idea an iota, so the Fallacy of Exclusion is used again.

Attenborough goes on with this interestingly constructed statement: "it's predictive" because "if we switch to a new land-based diet that's poor in those nutrients brain function will suffer". Notice the "true but false" nature of a statement like this; it's true *but* only when you throw in the phrase "that's poor in those nutrients". You can say "if we switch to a new ____-based diet that's poor in those nutrients brain function will suffer" and put *any* environment, including "aquatic" or "shore-based", into the blank space and it will be equally true. The statement is designed, whether deliberately or through not thinking it through, to mislead the audience into thinking that a "land-based diet" will *necessarily* be "poor in those nutrients". But we know this is not true.

First, we know that peoples who don't eat seafood can and do develop normal brains and brain function, so we know that fish is not a requirement for enough of the necessary fatty acids. We also have people who've calculated the amounts of energy and DHA and/or LNA fatty acids needed to develop normal brains and it's available even in savanna conditions.

Mentions the 2004 WHO report in regard to iodine deficiency.

Small amounts of iodine are necessary for normal brain growth and function, and iodine deficiencies do exist in some modern diets. Fish are a good source of iodine... as are plants grown in soil with enough iodine. Again, we know that peoples who don't eat seafood can and do develop normal brains and brain function, so we know that fish is not a requirement for enough iodine.

Attenborough mentions, for no particular reason really, the small hominid found on Flores last year, the one the researchers referred to as "hobbits". (I say "small hominid" because from what I understand at present there's only skull and a bunch of fragmentary bones and the situation doesn't seem to be as clear as it should be to declare that a bunch of small hominids were living there; that hopefully will be clearer up later this year (2005).)

Apparently the "hobbit" reference was simply to introduce Peter Brown mentioning that there are remains of *Homo* (non-"hobbit") from about 845,000 years ago on the same island of Flores. The general idea of how they got there, crossing a bit over 20 miles of water, was to drift on masses of vegetation or logs or possibly, but not as likely, lash something together as a crude raft.

Elaine Morgan sees it differently, of course, saying, "But if you're an anthropoid ape or some variation of an anthropoid ape you don't get to construct crafts unless you're very confident in the water already and quite happy with crafts. I also think that at that stage we would have been perfectly capable of swimming across."

Which might not have been a complete non-sequitur had the creatures in question actually been anthropoid apes. They were not "anthropoid apes"; they were *Homo erectus*, fairly skilled tool users for over a half million years. While it's unknown whether they could float across on logs and such (as many animals have done for more further distances) it seems likely enough for a hominid like *erectus*. If an iguana can do it, why should it be astounding that *Homo erectus* could do it?

On to the subject of holding one's breath (and, despite my desires at the halfway point in this half hour, I'll restrain myself from any puns on that at present).

Attenborough again: "in land mammals, including our primate cousins the chimps and gorillas, breathing is entirely involuntary; you can't get a chimp to hold his

breath. Breathing is as unconscious as the beat of its heart." I don't know if the ape part of that claim that is really true or definitely false -- Attenborough offers no evidence for the claim, but it might be true. However, we do know that some land mammals at least, dogs for instance, can hold their breath, and that untrained dogs can hold their breath somewhat longer than untrained humans. So the statement overall is simply untrue. The amount of fine control we have over our breathing is very unusual and is apparently a side benefit of habitual human-style bipedal locomotion. Not using the muscles of the thorax in locomotion frees them from that relatively involuntary chore, allowing them to be used in better breathing control, which helps a lot in advanced forms of speech.

Attenborough ignores these fact so he can advance the AAT/H view; he outlines it like this:

fully committed aquatic mammals such as dolphins -- conscious breathing
"fully aquatic, exclusively conscious; fully land-based, exclusively unconscious"
Humans, half and half. Dogs he doesn't mention; one of those ugly fact things, I guess.

Hoover the seal is brought in as evidence that conscious breathing necessary for speech: "It's said he was the only non-human mammal ever to make the sounds of human speech."

It may be "said", but the TV show *America's Funniest Videos* would disagree; they regularly have dogs and cats which make the sounds of human speech. However, Hoover was an unusual case -- he apparently did actually mimic a few phrases learned from his former owners -- common in some birds but something never before seen in any mammal, including any aquatic mammal. However, W. Tecumseh Fitch (you see him mentioned on my page re the larynx) is getting some people working on this, since it's an interesting possibility. However, it doesn't really mean that swimming and diving are prerequisites for speech, as Attenborough would obviously like to imply.

Now it's on to the "swimming babies" business.

Attenborough again: "Diving response and natural swimming abilities are displayed extremely early in the growth of the human infant. Indeed they are strongest at birth." If you haven't already, read my page on ["Swimming Babies"](#) for more info on this. What he's describing is not unique to humans or to non-terrestrial mammalian infants; it's a universal mammalian response, possibly a holdover from pre-mammalian ancestors. This is why I have that Darwin quote on my opening page; these "false facts" just never go away.

Attenborough then has Susan Burvill, consultant midwife at Addenbrooke's Hospital at Cambridge, mention the diving reflex, this time in a way that suggests it's something odd or in need of explanation in human infants: "when they're born they still have intact the diving reflex, or we call it the duck reflex, what that means is if there's water on the face, there's receptors on the face, that will close the throat off so actually the baby not inhale water." Rather than being something odd about humans or human infants, it's actually found in all mammals (and birds and reptiles as well), whether terrestrial or aquatic. Like the infant swimming response, AAT/H proponents here look at *a feature that humans share with all mammals* and are surprised that it should be so.

Various things about water birth, which Attenborough suggests "seems extraordinary" -- but why wouldn't a baby which has spent 9 months floating in the watery environment of the womb do fine going from there to a pool of water for a very brief period? Why would anyone find that "extraordinary"? Like the business of being surprised that we resemble our primate relatives instead of various distantly related ungulates, like being surprised at humans having features which are present in all mammals -- why are these people continually baffled by the expected?

Now we get into something interesting, and something I haven't researched thoroughly. It's the vernix caseosa, which is a white substance composed of sebum (the excretions of sebaceous glands) and sloughed off skin cells in babies which are either preterm or at term (it's gone in postterm babies). It apparently has antibacterial properties, and it was supposedly found only in humans.

Attenborough's research crew apparently found a researcher in Nova Scotia, Don Bowen (the way they put it makes it sound like Bowen found them), who says he's found something which he assumes is similar in newborn harbor seals, and to a much lesser extent in grey seals, but although "we always assumed that it was analogous to the vernix that humans are born with; we never pursued it systematically." Attenborough grabs this thread and spins a thick warm jumper out of it, but it would be nice to have enough threads to make at least a bit of yarn before you start spinning yarns. Actually, this is one of the basic problems in AAT/H techniques, now I think about it; not the worst one, certainly, but a problem nevertheless. Before spinning a story about it, wouldn't it be apropos to ask some interesting questions? And there are some fascinating questions that immediately come to mind:

A) is this feature in seals actually essentially the same as in humans -- sebum plus dead skin cells? (knowing what I do about seals' skin and glands, I think it likely is, but it would be sensible to know for sure before putting it forth as evidence.)

B) if despite people knowing about the vernix in humans it wasn't known to be present in seals until now might it not also be found in other mammals if one

searched hard enough? (AAT/H proponents often seem to make the mistake of assuming we know all the comparative angles between various animals, so if we don't know if a feature exists it means we know it doesn't. The two -- not knowing and knowing it doesn't -- are not the same thing.)

C) and might the feature not also be one of those things, like the hymen, which happen to be present in various unrelated mammals (humans, lemurs, llamas, guinea pigs, hyenas, elephants, rats, horses, some species of galago, toothed whales, seals, and sirenia) apparently by chance?

All of these are interesting, even fascinating questions, and so (naturally, it seems) Attenborough doesn't ask any of them, much less try to answer them. (This too seems to be par for the course with AAT/H proponents, I'm sorry to have to say.)

So, some form of vernix seen in a few seals... interesting? definitely yes. Evidence for an aquatic past? like the hymen, no, not really, not unless there's a lot more to it than meets the eye so far. But it is interesting and bears an actual thorough looksee. Doing that, however, is not the general method I've seen in AAT/H proponents -- they tend to throw something out in the ring (either factual or "false fact"), claim it's evidence, and if it needs further looking at to see if it is evidence or not, they expect someone else to do that. And that's what Attenborough did here with the vernix.

Update: I notice that Robert Yerkes and James Elder noted vernix caseosa being present in the chimpanzee; I've also seen reference in academic papers about the pre-birth inhalation of vernix caseosa in baboons. I'd like to see more info on this. You can find many categorical statements, often by medical scholars, that the vernix caseosa is not found in any non-human primate, but then a decade or so ago you could find the same kind of emphatic statement about the descended larynx, which we know is false.

Cuba, case 12, exhibited no fear of her baby or reluctance to handle it, but she did so awkwardly, often holding it head down. She was observed to bite one of its feet as if testing its edibility, and she scraped the lower back with teeth and lips so hard that it looked as if she were removing the skin. Actually she took off only a coating of vernix caseosa which overlay the hair.

Robert Yerkes and James Elder, pp. 46-47, "Concerning reproduction in the chimpanzee", Yale J Biol Med. 1937 October; 10(1): 41-48

Attenborough closes with the claim that "the evidence, bit by bit, and year by year, really does seem to be accumulating in its favor. Perhaps, like Wegener and his continental drift, the tide really is turning towards Alister Hardy and Elaine Morgan

and their proposal that our earliest human ancestors were born, not on the grasslands or the forest but in the shallows and the dunes on the margins between sea and land." Ahh, Wegener again; when he was alive the guy went around the world in the pursuit of his studies but I swear he gets out more now. Everyone wants to be Wegener, or at least the false story of Wegener they use. Well, it would've been nice if Attenborough had presented that "accumulating" evidence -- what about the many features he mentioned, the "standing upright, naked and sweaty, swimming and diving", the "strange hairlessness", the "layer of fat just beneath the skin", the "curious paradox that noses are very large but our sense of smell is comparatively poor", and of course "our bipedal locomotion on two legs" (not just bipedal but *bipedal on two legs*)? Well, maybe he's planning a sequel.

Just before winding up he quotes anthropologists Cameron and Groves saying "Morgan's latest arguments have reached a sophistication that simply demands to be taken seriously."

I agree. Read my site -- that's why I did it.

Brain size and EQ (Encephalization Quotient)

Some EQ comparisons and what *not* to do with them

The first set of EQs I'm using on this page are from a single source and will vary somewhat from other figures that may be found in other sources (as you'll see in the sets of EQs I present later on this page). For instance, in this first source human EQ is listed at 5.07, while other sources may use figures up to 8 plus for humans. Besides different methods of calculating EQ there may be some variation in the specimens used as source material -- most, if not all, animals vary from individual to individual in both body and brain size, and this can make a difference depending on what source specimens are used for comparison. We tend to have a fairly large number of specimens for animals like humans, and others like rats, because we've studied those animals the most -- this should make it possible to get a better idea of the range of EQ within those species compared to less commonly studied species. So remember to be very careful about comparing EQs obtained from different sources; different articles, papers, or lists may have EQs calculated in different ways. You'll see that the individual numbers may vary from different sources, but the general outlines of comparison, the relative numbers (of, say, humans compared to monkeys) are similar.

Also, as Schoenemann, in a 2004 article (P. Thomas Schoenemann "Brain Size Scaling and Body Composition in Mammals", *Brain Behavior Evolution* 2004;63:47–60), points out that: "They [These results] also suggest that some

measure of lean body mass is a more appropriate scaling parameter for comparing brain size across species than is overall body weight." (pg. 47)

This of course makes sense since fat varies according to food availability; it's a food source that's stored when times are good and used up when times are bad.

However, Schoenemann later points out that although using Fat Free Weight would be best, it's difficult to do and isn't all that much more accurate in practice, even though it's potentially better:

"However, because FFWT [Fat Free Weight] is much more difficult to estimate than WT [Weight], and because the differences in EQ's are likely to be small between the two methods, this is impractical even if theoretically more appropriate. There might nevertheless be subtle but important differences in EQ estimates, which could be significant in some cases given the vast range of FFWT and WT in mammals, and this should be kept in mind when comparing brain sizes across species." (pg. 58)

The effectiveness of EQ comparisons breaks down with very large (extremely large) animals -- very large whales fall into this category. For instance, the Sperm whale (EQ .28), Blue whale (EQ .15), and Humpback whale (EQ .18) are not thought to be exceptionally unintelligent, although they have very low EQs. Until you get into the larger whale-sized creatures (and after all these whales are 6 or more times larger than an African elephant) EQ works quite nicely.

Now take a look at these EQs and I'll talk about at what you *shouldn't* do with information like this:

Humans 5.07

Proboscis monkey 1.11

Red colobus 1.50

Patas monkey 1.93

Capuchin 2.52

various non-human primates ranging from 1.04-4.04 (not including lemurs, which tend to have EQs lower than 1 in his set; both lemurs and langurs bring the primate average down)

Bottlenose dolphin 3.60

various dolphins ranging from 2.43-4.45

Ringed seal 1.37

Manatees .32

Caribou .78

Wildebeest .68

Warthog .40

Hippopotamus .27
African elephant .63

Armadillo .26
Opossum .39

First off, too bad about the armadillo; good thing they're cute and can't bite. Then let's look at these numbers from an environmentally deterministic view, like the AAT/H typically uses. Remember, it's *not* a view that should be used, but it's instructive.

Look at the numbers for hippo and elephant; the semi-aquatic mammal has a much lower EQ. Look at the grouping of herbivores which includes the manatee; the aquatic manatee has a dramatically lower EQ. Look at the monkeys; the one which uses the environment that AAT/H proponents say may be similar to the one we used (the proboscis monkey) has, by a large amount, the lowest EQ. The savanna-dwelling patas monkey has a much higher EQ. Now if you were using the environmental determinism of the AAT/H, you would be forced to conclude that aquatic environments are horribly unlikely places to find higher EQs and that savanna environments are great places to find them.

But I've already mentioned that using this sort of environmental determinism is a bad idea; it just doesn't make sense (and these numbers should help anyone, even AAT/H supporters, understand why it doesn't make sense -- maybe they should consider not doing it anymore). It's not that you can't find some correlations at times -- the problem is just the reverse; there are always a great many correlations all over the place. But finding a correlation is not finding a cause.

This is well known in science, so well known that there's a maxim about it: *correlation is not causation*. As an example, let's say you look at the reading abilities of 5 year-olds and 10 year-olds, and you measure their height too. Guess what, they correlate extremely well -- so does increased height cause increased reading ability? Does increased reading ability cause increased height? Or are both due to some other factor or factors? Anytime you see a correlation you have to look past the correlation if you looking for the cause -- the correlation is not necessarily the cause. It might be in some cases; it often isn't. And be sure to check to see if the correlation is actually accurate too.

I've already mentioned (on the BBC page) what actual correlations can reasonably be seen as causes for these EQ differences: predator species generally have higher EQs than prey species, prey species which use active predator-avoidance strategies generally have higher EQs than prey species which use passive predator-avoidance

strategies, and animals which live socially, especially in relatively small groups with a lot of individual interaction, tend to have higher EQs than other species.

This is because, due to selection pressure during evolution, the more a species needs to think, the higher their EQ. But larger brains are not necessarily better, because the larger the brain, the more energy it takes to develop it and feed it. So animals tend to develop the size brain they need and that's about it. They are not, as Attenborough suggests, just waiting for the right diet so they can grow a bigger brain and outdo their competition.

By the way, you may have noticed that the AAT/H is largely made up of correlations, many of them inaccurate or imagined. (As seen in common questions put forth by AAT/H proponents such as "where do we find animals which are (fill in the blank)" -- where the "blank" is fat. or hairlessness, crying emotional tears, sweating, having breath control, etc.) But even if the correlations were real, they still aren't the evidence AAT/H proponents seem to think, just as the very real aquatic vs. savanna correlations I showed above are not evidence for the "savannas are better than aquatic" conclusion I put forward (with tongue in cheek). Likewise, Crawford's "aquatic is better" conclusion is not supported by the correlation he mentions, even if the correlation were accurate and not an inaccurate "apples to oranges" (predator to herbivore) comparison. His conclusion is damaged even more because his correlation isn't even accurate.

Let me show you one more example of why AAT/H proponents can't (and really shouldn't try) use brain size as evidence because of it's supposed correlation with the habitats and lifestyles they say the "aquatic ape" used. Note that these EQs are from a different source than those above and shouldn't be directly compared with those EQs, since they were calculated somewhat differently -- humans, for example, are listed at 6.28 or 8.07 rather than 5.07 as in the above set of figures -- always be careful about comparing EQs from different sources.

In the following sets of EQs the species AAT/H proponents most often point to as occupying a habitat and living a lifestyle like that of our supposed aquatic ancestors are bonobos and proboscis monkeys. The species which occupy savannas (besides humans) are baboons and patas monkeys. Note that bonobos, although they are demonstrably quite smart animals, have lower EQs than their close relatives the chimpanzees and humans. If one was to use the environmental determinism regarding brain size that Crawford, Cunnane, and other AAT/H proponents say we should use (and keep in mind that I *do not* suggest this is a good idea; just the opposite) we would be forced to conclude that a semi-aquatic lifestyle to the degree of bonobos is bad for brain growth. Likewise, when we look at the proboscis

monkey and compare it to other monkeys, such as the guenon, mangabey, macaque, patas monkey, or baboon.

Species	EQ -- from Martin 1984	EQ -- from Jerison 1973
<i>Homo sapiens</i>	6.28	8.07
gibbon	2.40	2.60
common chimpanzee	2.38	3.01
bonobo	1.80	2.36
macaque (<i>Macaca</i>)	1.78	1.95
mangabey (<i>Cercocebus</i>)	2.09	2.29
guenon (<i>Cercopithecus</i>)	1.96	2.05
proboscis monkey (<i>Nasalis</i>)	1.07	1.24
Colobus	1.27	1.41

baboon (<i>Papio</i>)	1.74	2.05
patas monkey (<i>Erythrocebus</i>)	1.99	2.19
capuchin monkey (<i>Cebus</i>)	3.25	3.25

Now these conclusions do not hold, of course, because this sort of environmental determinism, as practiced by AAT/H proponents, is BS, to be blunt. But since they use it, one might wonder why they don't mention these uncomfortable conclusions which inevitably follow from it.

By the way, check out the forest-dwelling (and tool using) capuchin -- it just throws the final monkey wrench, so to speak, into the AAT/H-style environmental/dietary deterministic strategy for explaining brain growth. Just don't use that strategy; it doesn't fit the facts.

Jerison, HJ, 1973 *Evolution of the brain and intelligence*. Academic Press, New York.

Martin, RD, 1984 "Body size, brain size and feeding strategies", in *Food acquisition and processing in primates*. Chivers, D; Wood, B; Bilsborough, A, eds. Plenum Press, New York.

Schoenemann, P. Thomas, 2004 "Brain Size Scaling and Body Composition in Mammals", *Brain Behavior Evolution* 63:47–60

Review/Critique of BBC/Discovery Channel documentary *The Aquatic Ape*

In 1998 the BBC did a television documentary on the aquatic ape theory, which was also broadcast in North America on the Discovery Channel with the script narrated by Betsy Ames instead of the BBC's Andrew Sachs. The script was changed only slightly, a phrases here and there, and several short sections dropped to cut about 6 minutes from the North American version. This critique uses the fuller BBC version. The show is available online in various places that host videos; since the

BBC could complain about that I won't post any links, but it should be easy to find with a simple search. The BBC version is generally available chopped into five segments, which is what I refer to here when I mention specific times in the broadcast; the North American Discovery Channel version I've seen online in one large file.

Other than the narrator, both versions have exactly the same credits, producer, editors, etc. Apparently no one wanted to be associated with the creation of the script itself as no writer is mentioned in the credits. It's supposedly a balanced view -- it has (very short) sound bites from four critics of the idea -- but watching it one is struck not only by the fact that no serious challenge is made to most of Elaine Morgan's very challengeable claims, but by the way the editing and the visuals are done to further undermine this supposed balance. Since so much of a documentary film or video is the visuals this is critical; for instance the script for this 50 minute program is less than 7,000 words, only about the length of a longish magazine article. This critique is slightly longer, in fact. So the visuals are a huge component of what you take away from a documentary, and because of the nature of visual information what they add is often not processed as critically as words are. This is something you see to a certain extent in almost every documentary, so this one becomes a good lesson in what to look for when you watch one, since this one is so obvious about it.

Overall remarks -- things to watch out for.

Early on the program's theme is set: "a housewife" confronting "the scientific establishment". Various times throughout the program Morgan is referred to as a housewife (4 times) or grandmother (once) or in one case reference made to her then "advanced" age. Also notice how the interviewees are photographed: When Elaine Morgan is interviewed she's shot full face while almost everybody else (except Lee Berger, who moves a lot) is shot at three quarter front angle; Morgan often seems to be looking slightly to one side at the interviewer but her face is fully facing the camera. Also, everybody who is critical of Morgan's ideas seems to have been photographed either at a distance and outside and their eyes are squinting and in shadow (Leslie Aiello also has sunglasses on) Morgan is the only person whose eyes can be clearly seen. This technique makes it seem that she is being honest and forthright with you, the audience, while the others, who aren't facing you or making eye contact (their eyes are hidden) are not.

Throughout the program check out the contrasting visuals used when talking about aquaticism and when savanna is mentioned. Aquaticism is matched with many contemplative shots of cute "swimming" babies (actually floating underwater), while

"savanna" or "hot savanna" is often matched with shots of stalking and attacking lions. So when the word "savanna" is used, it's almost always matched either with those lions, with the words "hot" or "dry" or "arid" or "open" or "plains", or with predators or some form of the phrase "savanna theory" or "savanna theorists", or some combination of those.

On to the show:

The narrator sets the central premise of the Happy Cute Wet Baby Theory:

"Human babies underwater. Instinctively they swim. they know how to hold their breath. They're perfectly relaxed. Body fat keeps them afloat. They seem to be at home in the water. None of the great apes at this age can swim, float, and hold their breath. How can humans, why can they, where did this natural intimacy with water come from?"

Sorry to be a downer right on top of all those opening shots of cute wet babies, but did you know that the Center for Disease Control (CDC) in the USA reports that "Fatal drowning remains the second-leading cause of unintentional injury-related death for children ages 1 to 14 years"? That children ages 4 and under drown at a rate several times higher than other children? That according to the USA's National Safety Council, "Of all preschoolers who drown, 70 percent are in the care of one or both parents at the time of the drowning and 75 percent are missing from sight for five minutes or less"? That (back to the CDC), "It is estimated that for each drowning death, there are 1 to 4 nonfatal submersions serious enough to result in hospitalization. Children who still require cardiopulmonary resuscitation (CPR) at the time they arrive at the emergency department have a poor prognosis, with at least half of survivors suffering significant neurologic impairment."?

Maybe those babies shouldn't be so "completely relaxed", especially since, you might note, at no time does any baby show himself capable of raising his head above the water level for the purpose of breathing, to paraphrase the finding of the study I mention below. In the shots used in this program, notice how you have to look carefully to see that infants are being helped a great deal by adults, especially when their heads appear above water. Most of the shots used disguise this.

As my short page on the "[swimming babies](#)" reference mentions, this ability the show suggests is unique to humans was found in every mammalian infant tested; the researchers found the breathholding and "rhythmical movements of the human infant are quite similar to those of other young quadrupeds in water". It is apparently an ancient reflex shared with all mammals, and not surprising in

mammals which after all have at that point spent most of their lives floating underwater in the womb. The study also found that "at no time did any baby show himself capable of raising his head above the water level for the purpose of breathing".

Rant alert: After the opening titles, the shots of heat waves and attacking lions that accompany mentions of "savanna" on this program, the narrator does one of these: "This search for our origins has been a long and bloody battle. Careers and reputations have been built and demolished with each new discovery, and each one redrawing the controversial path from ape to human."

I'm *really* not a fan of this overdone "demolished", "redrawn" etc. style of science presentation, but it's virtually always done in popular writing, presentations, and of course radio and TV. A more truthful way of stating it is that paleoanthropology, like *every other field of science*, is always being edited to become more accurate. That doesn't capture attention as well as the notion that all previous knowledge has been overthrown, so it's common to suggest the latter. This is so even for press releases about scientific work, and I think it does science a great disservice. For one thing, it suggests that science really doesn't know anything, ever really find anything out, because each new discovery just throws the old stuff out the window. That's not true, but can you blame people for thinking that since that's how new information, studies and discoveries are presented to the public. For most people this just breeds confusion, but in the most extreme cases it helps allow pseudosciences like creationism to sell their view of science as hopelessly broken, to which they offer their snake oil alternative. Sorry about the rant; it's a bit off topic, but really, keep this in mind whenever you're faced with a popular account of science, whether in books, TV, radio, or news.

More on topic, note the techniques in this opening, the things I mentioned right at the top: the "hot dry and waterless", punctuated with blowing dust and waves of heat, the film of attacking lions, all contrasting starkly with lovely, cute wet little babies. Which theory do you like better so far, the "Wet Happy Peaceful Babies Theory" or the "Hot Dry Waterless Theory with Attacking Lions"?

After that you need a transition, so they dutifully stick in about 10 peaceful seconds of chimps in a forest before introducing Elaine Morgan; wouldn't want her entrance to be too closely associated with those lions.

These clips introducing Elaine Morgan set the stage for the premise of the show: "a housewife" confronting "the scientific establishment"; this basic setup will be repeated to hammer it home throughout the show. (By contrast, only once more will they refer to her being a successful career BBC television fiction writer.) Note too that as soon as she mentioned savanna again they went right off to some shots

of a treeless arid area of savanna even though these shots, featuring modern day savanna herdsman, don't have any reasonable connection to the period of human evolution in question. They're just there to cement in your mind early that when you think "savanna" you should think "hot" dry "arid" "treeless", and not, for instance, savanna woodland as has been talked about in human evolution for decades.

Morgan furthers this strawman when she says that human characteristics "cannot all be accounted for by saying they crossed open spaces of grass between the trees". That's a correct statement and would be a good point if that was actually what scientists say. It isn't. The hypotheses that paleoanthropology has used (even the old and long-discredited "killer ape" idea) dealt and deal mostly with food, food-getting, and social interaction. Naturally they mention the environments we used and expanded into, as any theory should, but they were not the sort of environmentally deterministic idea that the AAT/H is, or the "savanna theory" as promoted by Elaine Morgan to be her strawman opposition.

And when Morgan says "Most of these features have parallels among aquatic animals" she's wrong; she says this often, and in fact this is the central premise of her work. But when you look at these features, what they're actually like in both physiology and life history, they are not the same and so are not parallels. Of course without these supposed parallels her work already falls completely apart. Notice that nowhere in this program -- although it's supposedly a balanced look -- do they present anyone seriously countering this mistaken central claim.

Also notice that when they mention these features with supposed parallels among aquatic mammals just which aquatic mammals they're talking about is -- pointedly -- not mentioned. This is something Morgan has been doing for decades, and it's a tactic which has been largely followed by the idea's other proponents. They prefer to coyly intimate that "aquatics" have these features, or that they're found among "aquatic mammals" as was done here, and this leaves the impression that we're talking about most aquatic or semi-aquatic mammals, or at least a great number of them, presumably a majority. But they're not; the features they're talking about appear only in whales, sirenians (manatees and dugongs), and seals (and for hair only in whales and sirenians), all of which are fully aquatic mammals which have been fully aquatic for tens of millions of years, far longer than hominids of any kind have existed. Their descriptions of these features is wrong, so they aren't even correct when it comes to the supposed similarities, but by not mentioning the mammals they're talking about they keep people from thinking the obvious question: how much like a manatee, seal, or whale am I really?

At the 4 minute mark, after the map of Africa shot, note the jump from "the cause" 15-20 million years ago to "the result": hominids at 5 million years ago; and note too the line "Between 15 and 20 million years ago there existed an ape that was the ancestor of all today's African apes and humans". This is one of several times during this documentary the script obscures what was thought to be the date of the transition from last common ancestor to hominid. I'll bring this up with a bit more detail later when this pops up again later in the program.

Also note the way Dart's 1960s era interview is setup to suggest that the decades since the 1960s have been dominated by Dart's views even though this is not true ("in the years that followed, many scientists then built on Dart's theory"). Dart's idea gained currency in the 1930s and peaked in the 1950s and early 1960s with the heavy promotion of a popular book by a dramatist, Hollywood scriptwriter Robert Ardrey. By the late 1960s and early 1970s (when Morgan came on the scene) the work of people like Thelma Rowell, Richard Lee, and Sally Linton had already cut the then somewhat wobbly legs out from under Dart's and Ardrey's killer ape idea; the interview by Dart shown here was, along with Ardrey's later books, by then pretty much a rearguard action for a near dead idea. Interesting that Ardrey isn't mentioned (in this program or anywhere in Morgan's writings, as far as I know) despite his importance in popularizing the killer ape idea; perhaps the fact of a popular book by a dramatist pushing a thinly supported idea was uncomfortably close to Morgan's own situation.

Between 0:08:27-0:08:51 on the first program segment we see again the appearance of attacking lions in conjunction with the words "on the savanna".

As the second program segment opens Morgan gives wading a little twist when she says "all primates if they're in a situation where they need to cross water will stand up on their hind legs and not only stand on them but walk on two legs". This is true if they do really want to cross deep water and *have* to stand upright; Morgan puts a little spin on it to suggest that they always do, which is not true. And I mean *really* not true, as in far from the truth.

Morgan offers a couple other twists when she says "If our ancestors had taken to walking on two legs how did they cope with the burden of more than one child, especially if they were a naked ape?" Last things first; it has always seemed likely that our body hair characteristics arose not at the transition from ape to human -- which is the period Morgan has always suggested for her aquatic period -- but later, probably around the time of *Homo erectus*, with its taller, rangier body and limbs showing signs of better adaptation for very hot and dry living, and this has lately been given strong support from genetic studies. This would mean that australopithecines were very likely to be hairy, probably not unlike chimpanzees, giving infants something easy to cling to. In addition, in terms of relative

helplessness and therefore degree of early grasping strength, australopithecine infants were extremely likely to be more like chimpanzee infants than modern human infants; the change was very likely at the time we see the massive change in brain size, which is around the time of early *Homo* millions of years *after* the time of Morgan's purported aquatic period. So an australopithecine mother would almost certainly be able to do just what's shown in the shot they use when Morgan makes her claim: a chimp walking bipedally with a child clinging as a child could cling to an australopithecine. These facts don't help the AAT/H case, so Morgan ignores them and slides these changes back about 4 million years to try to make her case. But really, even *with* Morgan's unlikely 4 million year fudging, what exactly *was* this burden for australopithecines? More than one early hominid child would almost always be, like both chimps and human gatherer-hunters, spaced out births, so you'd have a baby and a walking child. And of course a habitual biped could easily help support an infant with one arm and still have an arm completely free. So where was this insurmountable problem? Not said; just implied there *must* be, because Elaine Morgan says so.

Just after 3 minutes into the second segment Desmond Morris gives the account of how Hardy first thought of the aquatic ape theory. Hardy was struck by the notion that human fat was like marine mammals' fat. The narrator says "it was a revolutionary thought, one that at the time had no chance of being accepted"... which of course it shouldn't be, because it's wrong, human fat isn't like the blubber of marine animals either in shape or life history. At 0:03:55 Alister Hardy himself appears to provide the standard reason why he didn't pursue this idea at all for the next 30 years.

Alister Hardy: "Well, that was nearly 60 years ago. It seemed so preposterous though. I kept it dark. I looked to become a fellow of the Royal Society. And, I couldn't do that holding the aquatic ape theory. And so, quite candidly, I kept it swallowed up."

This is the story which has been repeated a great many times, but after thinking about it to me Hardy seems a bit like evangelist Aimee Semple McPherson and her classic "that's my story and I'm sticking to it", because when you look at Hardy's life, his work, and some of his other wild ideas -- about which he had no problem being forthcoming -- the story doesn't add up. Hardy says he got his aquatic ape idea in 1930 (he didn't present it anywhere until 1960 when he did so in a talk to a local SCUBA club). Let's grant the possibility that Hardy didn't want to do any controversial work before he got his FRS (Fellowship in the Royal Society, a high scientific honor). He got that in 1940. What was his excuse for the next 20 years? He wrote "Telepathy and Evolutionary Theory" for the *Journal of the Society for*

Psychical Research in 1950. That isn't controversial? In the 1950s he wrote other stuff about the paranormal and his belief that it influenced biology and our evolution; that wasn't controversial? Really? The notion that fear of controversy made Hardy hold back his aquatic theory when he was writing on far more controversial ideas doesn't add up. So what was his excuse for the 1950s? Hey, he even got a knighthood in 1957. What was his excuse after that? With all that far-out, far-fetched work he was presenting for decades when he supposedly was fearing for his career, where exactly was the problem with his reputation?

At 0:07:12 in the second segment the narrator says of hominid babies, "and if they let go, that was no problem; they could hold their breath underwater, or float on the surface". Keep in mind those statistics on drowning I mentioned at the start and the fact that those "swimming" babies are unable to raise their heads to breath; the program is not just wrong but getting close to advocating something dangerous. Also keep in mind that we don't know for sure when hominid babies started being born with lots of fat, but it's not likely to have been the transitional period Morgan is talking about. Since it is tied up with postnatal brain energy use during infancy (as I'll get into further along in this critique) it was extremely likely to have been when that brain size increased dramatically, probably either at the rise of *Homo* 1.5-2 million years ago or possibly even at the rise of *Homo sapiens* over a million years later than that.

Another problem; the program, describing Morgan's first book, says "Like sea mammals, but unlike all land mammals we developed an insulating fat layer and lost most of our hair". The problem here is that both of these statements regarding sea mammals are false. First, the fat they have is not, repeat not, an adaptation for insulation, as shown by Caroline Pond -- I mention this later again, because it comes up repeatedly, and I also talk about it on my page about fat on this site. It plays a minor role, but since even in very fat cold weather animals some areas of the body are very thinly covered, it simply can't have been adapted for insulation. Also, if it were adapted for insulation we'd surely see the fat near the skin used up last in cold weather, but it's the opposite. Then there's the hair business, and this runs into the GIGO (garbage in, garbage out) problem that the AAT/H virtually always runs straight into every time they talk about hair. First, the vast majority of aquatic and semi-aquatic mammals have lots of hair, including sea mammals. The exceptions are whales and sirenians (manatees and dugongs). Humans are not hairless; compared to apes what we have is shorter and finer body hair but just about as much, longer head hair, tufts of hair around the genitalia and armpits, and for that matter a great many humans have quite a lot of body hair overall. Humans' hair characteristics do not resemble the hairlessness of whales and sirenians at all,

and that's even before you take into account life history; when you do that you see that humans also -- unlike whales and sirenians -- undergo massive changes in hair characteristics right at puberty. Proponents of the AAT/H, including this program, always ask why humans' hair characteristics are different from apes and from savanna mammals; *they never ask why humans' hair characteristics are also different from every aquatic and semi-aquatic mammal on earth.*

0:07:50: Elaine Morgan here says "one objection to the aquatic ape theory is that people say primates are afraid of water"; she claims it was an objection by *critics* that primates stayed away from water when *actually that was a claim by AAT/H proponents which was used as support for the AAT/H*, until a critic (me) pointed out the facts. The false "fact" of non-human primates, apes especially, staying well away from water was used heavily as evidence FOR the AAT/H by its proponents until I pointed out the articles and studies showing that it wasn't true, at which point the actual facts became evidence FOR, even though they were 180 degrees opposite from what had been previously considered evidence FOR. There's a fuller explanation of this problem (it's a version of the *ad hoc* fallacy) in the "Theory leading the data" section on my [Summary](#) page.

Narrator: "In the 1960s few primates were known to live in association with water, but in the flooded forests of mangrove swamps of Borneo lives the proboscis monkey. This primate not only wades from tree to tree but is also a strong swimmer and can even swim underwater."

Crab-eating macaques are also strong swimmers and can swim underwater. Interesting question: how can they do this if -- as AAT/H proponents and the program state -- conscious breath control is required for doing so and non-human primates don't have it. (Dogs can hold their breath too, in fact untrained dogs can do so somewhat longer than can untrained humans.) In fact all macaques apparently swim well, even though most don't live in environments where they swim. Like proboscis monkeys they are hairy and quadrupedal; and all macaques, no matter their diet, are similar in brain size; proboscis monkeys' brains are on the lower end of the monkey scale. Where's the hairlessness, the uprightness, the huge brains, etc. that the AAT/H says come as an outcome of this contact with water?

At 3 minutes into the third segment the program, and Elaine Morgan, bring up the old Danakil Alps notion again, which has a couple of huge problems. One is that this puts the purported aquatic ancestor in salt water, and this doesn't fit our physiology at all; in fact it's directly and forcefully contradicted by our physiology, although Morgan spent many years claiming that this was not so. Also the notion of highly effective semi-aquatic mammals being stuck for millions of years on a near offshore *island* has always amused me. Perplexed me a bit too.

0:03:40: The narrator says about the mid-1970s DNA studies "with this new source of evidence scientists were able to pinpoint the split from apes, not from bone fragments but from DNA. What they found was that the split happened about 5 million years ago, shortly before the time of Lucy, and of the fossil footprints."

The program here is claiming that the date of the transition from last common ancestor with apes to hominid was unexpectedly changed by the use of DNA studies. This is a smallish point in my critique, but what they're trying to do here, I feel, is discredit the actual paleoanthropology that was being done at the time -- make it look so bad that anything, even a leaky theory, looks good by comparison -- plus change history to support Morgan's view of paleoanthropological science as a close-minded establishment that moves in lockstep. Early on the program had given the date of the transition as 15-20 millions years ago, and already when talking about Lucy they suggested that had changed the date, but apparently forgot they'd done so by the time they got to this DNA section less than 4 minutes later. The notion that the split between apes and humans was very old was current in the early 1970s in the UK and somewhat on the east coast of the USA, but for years before the DNA work was done the "west coast school" in the USA -- particularly Sherwood Washburn and his colleagues and students -- had held that the split was much more recent. This in fact this is what sparked the DNA research in question -- it's no accident that the research was done at the west coast schools largely with Washburn's colleagues and students -- and that research supported Washburn's and colleagues' date for the transition, a date that they had deduced based on the fossil evidence. (In fact Washburn's students were instrumental in other aspects of changing what paleoanthropology thought, for instance the active role of females in evolution, of female-child interaction, the important role of gathering in our past, etc.) Some scientists certainly did have their ideas changed by the DNA evidence, particularly those in Europe and some on the east coast of the USA, like David Pilbeam, but Washburn et al. had their ideas confirmed rather than overturned. The program here is doing a small thing to obscure the facts, because the facts point out how well paleoanthropology was doing, how much disagreement there is in the field (as there is in any field of science), and how science is perfectly willing to entertain, and actually demands, challenges to its ideas. And that just doesn't fit with the notion of a closed-minded establishment which spurns new ideas, which is what this documentary, following Morgan, has set as its theme.

At 0:07:43 in the third segment Morgan says: "The kind of questions that the savanna theory was completely failing to answer were particularly questions to do with the soft tissue like the skin the fat and flesh. They're unable to explain why we're the only savanna animal to have lost its fur and why we're the only savanna animal to have got layers of fat under the skin."

Couple of points, about both facts and tactics: humans indeed have no fur, but this is just like apes, what they mean is much shorter body hair but "no fur" sounds more dramatic and more of a change. Also note that when you don't say "no hair" or "hairless" and instead look at just exactly what our body and head hair characteristics are you find there's another group of mammals which have no member whatever sharing those characteristics -- aquatic and semi-aquatic mammals. This is why AAT/H proponents, and this program among them, makes a point of mischaracterizing those characteristics again and again and again; stating them accurately shows their claim that aquatic mammals share them is bogus. None of them do. Not one, anywhere. Looking accurately at those hair characteristics and comparing them to our near relatives what you find is that among primates there is a progression in number of body hairs, with apes and humans having fewer than monkeys, and the difference between humans and chimpanzees is not so much number of body hairs, but length and thickness, a difference caused by relatively minor genetic change, so minor we see almost as much variation among people today as between chimps and humans.

And our layers of fat are just like other primates which are allowed to get fat. Caroline Pond, as I explain on my [page about fat](#), has pointed out these facts about fat and the evolutionary significance of these facts. And the combination of sexual selection, Peter Wheeler's Radiator Hypothesis, and the recent genetic evidence regarding the likely timing of hominid body hair change explain that feature very well too. Bottom line here is that Morgan's theory, on the other hand, fails to answer these questions, and is doomed to fail because the idea's proponents state the questions incorrectly right at the outset. This is the GIGO problem (garbage in, garbage out); if you base your questions on false claims it's guaranteed you will fail to find the solution to the problem you're trying to answer.

There's a few interesting things when Marc Verhaegen pops up at 0:08:08 in the third segment. First is the description of him as a "Medical Scientist". He's a medical doctor, a general practitioner, which is also in the label they give him, but given that he's ludicrously wrong about so many facts -- even facts about human medicine -- it seems absurd to hang "Medical Scientist" on him. My [page on him](#) has various examples, and they're just a few of a great many; I don't think any AAT/H proponent says as many wrong things per word as Verhaegen does, whether it's "rhinos are predominantly aquatic" or that ear exostoses "occur exclusively as a direct result of long-term exposure to relatively cold water". That last, and several other decidedly odd things he's said online, are things which should conceivably fall within the area of expertise of even a general practitioner, but Verhaegen either doesn't know they're false or for whatever reason just says them anyway. That's

not the sign of a very knowledgeable medical doctor, much less a "Medical Scientist". Considering only what he says in this program, Verhaegen saying "the anthropologists have a quite different view on this, they see only their bones and they find them in dry environments" suggests that paleoanthropology looks only at present-day environment of fossil deposits and assigns it to the past, which is ludicrous. His segment also helps the program ignore (and therefore hide from the viewer) the fact that paleoanthropology does look at comparative anatomy of modern humans and other primates, as well as other animals (and of course primate behavior) -- not just fossils as they're suggesting here. He claims he finds more support for his ideas among medical doctors than among anthropologists, and this I find believable. Medical doctors are generally unfamiliar with the details concerning human evolution and subjects like the characteristics of sea mammals (the topic of seal sweat rarely comes up during a blood pressure check), so they're far more likely than anthropologists to be supportive of a theory based on false analogies with these characteristics and misunderstandings and falsehoods concerning human evolution.

An interesting example on the media critique front is the visuals of ambulances, emergency rooms, and intent surgeons that appear when they bring in Verhaegen; they also cut to several shots of the intent surgeons during Verhaegen's interview. This is almost as ham-handed whack-you-over-the-head as the rampaging lions business: get it? This guy will save your life! You have to trust him! So overdone it's funny really.

At 0:09:04 in the third segment of the program there's yet another appearance of rampaging lions right when "hot savanna" is mentioned -- I'm sure it's just a coincidence. This gets downright funny -- okay, I have an odd sense of humor, I admit -- when the narrator gets to talking about waterholes and rivers. The lions are pictured there too -- the only time any type of predators are shown in any connection with narration about aquatic environments -- but now they're just peacefully sitting by the river's edge having a drink. You see how peaceful water is? Even lions switch from ferocious beasts to tame tabbies in casual contact with it. Which theory will you believe now, the documentarians are asking you, the "Tame Tabby and Cute Wet Babies Theory" or the "Fierce Lions and Hot Arid Waterless Treeless Plains Theory"?

That narration talking about waterholes and rivers also presents a falsehood. This is the claim about hippos being particularly fatty. They're roundish, that's for sure, but they're also actually quite lean. Their relative hairlessness, which the program claims as an aquatic trait, seems to be for the same reason we see it in elephants and rhinos -- thermoregulation. As large animals with a lot of volume to surface

area (because of that roundish body and relatively short legs) they have trouble ridding themselves of heat. This is simple physics -- more surface area to volume gets rid of more heat, less surface area to volume (the elephant, rhino, and hippo problem) holds in more heat. You can also see this idea tested and confirmed in the fact that the cold weather ice age versions of both rhinos and elephants were hairy. In fact this thermoregulation problem may be why whales and sirenians are hairless too; among mammals they have the highest volume to surface area ratio of all. Back to the "hippos are very fatty" claim -- the fact that this claim was false was pointed out to Elaine Morgan before the summer of 1995, some 3 years before this documentary was released (and 3 years before at least some parts of it were filmed). Yet this false fact, as Darwin referred to such bits of misinformation, has endured long, as Darwin pointed out such things do.

Also, as described on my [page about fat](#), Caroline Pond has explained why we can tell that fat has not been primarily adapted for insulation during evolution, counter to the program's claims at this point. Not just our fat, everything's fat. It plays a minor role, but since even in very fat cold weather animals some areas of the body are very thinly covered, it simply can't have been adapted for insulation. Also, if it were adapted for insulation we'd surely see the fat near the skin used up last in cold weather, but it's the opposite. No wonder Adrienne Zihlman thinks, in the words of the narrator, "the aquatic ape theory makes too much of this". She thinks that because the AAT/H claim just doesn't fit the facts.

So trying the approach of throwing a bunch of stuff at the wall and hoping something sticks, Morgan and the show try to switch the talk to only human babies. Okay, go ahead, throw out the adults, throw out the children, who if not overfed typically go through the leanest period of human life, just talk about babies. This fatty baby thing is unique to humans, which means it's also unlike aquatic mammals; why are they not mentioned? Answer? Because the obvious reason for this baby fat is not environment, not aquaticism, but the large amount of postnatal brain energy use during infancy, which is also unique to humans. ("Humans are unsurpassed among mammals for which data are available in the size and energetic cost of their brain, and this feature is pronounced during infancy, when the brain consumes an estimated 50–60% of the body's available energy." pp. 202-203, Christopher W. Kuzawa, "Adipose Tissue in Human Infancy and Childhood: An Evolutionary Perspective" in *Yearbook of Physical Anthropology* 41:177–209, 1998)

Two unique-to-humans things, one (lots of baby fat) required for the other (the brain's extremely heavy energy requirements during infancy); why do AAT/H proponents not see this obvious linkage?

Interestingly, the energy requirements of the brain in human infants is not due to brain growth per se, as is often thought -- I thought so, and first wrote this section

with that in mind before I checked it -- it's just a big organ, relative to body size, and it's inflexible in its energy requirements. The Kuzawa paper I reference above concludes that the fat stores of human infants built up just before and continued after birth have evolved to deal with periods of relative malnutrition, both in the transition from placental to breastfeeding and later during periods when the mother may be malnourished (which shouldn't happen in well-off modern societies but of course was common in the more nutritionally stressed prehistoric societies). The energy from these fat stores also helps stave off illness during infancy and these times of relative malnutrition.

Besides the fact that it pointedly ignores the obvious and well-supported answer, notice how the program's AAT/H argument *was* fat as insulation, then suddenly switches to fat as buoyancy when the insulation claim is shot down. Notice too how fast the program moved to throw out all humans except babies when talking about fat. These are an example of the *ad hoc* logical fallacy in action, as the AAT/H supporter flails about looking for a way to ignore valid criticism. This also puts the critic on the run checking out yet another new claim; the proponent can come up with new claims far quicker than anyone can counter them, since countering them takes research time and energy. Each time the critic provides the actual facts about a claim, the proponent simply changes the claim while ignoring the criticism; often the proponent will then bring up the old claim later as if it hadn't been shot down. In this program you see only a snapshot of this tactic, but it does illustrate the method.

Fourth segment, 0:01:22: They now get into conscious breath control. Monkeys and dogs, at least, are able to swim underwater without this conscious breath control ability, or much of it (do they have it at all?); therefore why think it's necessarily aquatic? Just because Elaine Morgan says so, it seems. Actually, it's pretty clearly due to fairly simple mechanical reasons -- the uncoupling of locomotion from the front limbs (our arms) due to bipedalism. It's a side effect that turned out to be very handy for other reasons. As for baby fat for buoyancy, the aquatic ape then needs to be mighty non-aquatic for childhood when humans who aren't overfed typically go through the leanest period of their lives (after that huge spurt of postnatal brain growth is finished). This is one of those claims on which the AAT/H is internally inconsistent, as I point out on my [Summary page](#).

The next two clips (fourth segment, 0:02:27), of opponents, are interesting especially in what the sound bites the producers use *don't say*. Leslie Aiello says it's "a bizarre idea" and that "there isn't any evidence to my mind in the fossil record"; now certainly this short clip isn't all she said, but from this we get no sense of why she says it's a bizarre idea and why we might expect to find fossil evidence that we

don't find. Either the producers got someone who couldn't explain that or they cut out anything she said that did explain it. It's somewhat similar with Lee Berger, nothing really said in his clip except opposition; it makes it seem a) as if he's being nitpicky about "the way it's argued" as if it were just a formatting problem or something (her margins aren't wide enough!) and b) as if he's being whiny about AAT/H proponents' reactions (when he's being very accurate). This leaves the impression that Aiello's and Berger's opposition is based on personalities or whims or stubborn closed-mindedness. That, of course, is the theme the show is presenting, and the one that Morgan has long promoted. So view the clips of opponents with that in mind.

[NOTE: Owen Lovejoy is presented (fourth segment 0:04:01) from an old interview saying how awful bipedalism is in virtually every way. This is so incredibly overstated I have to wonder just what was the context. I haven't been able to find the original interview anywhere so far so I don't know what else Lovejoy said; what's presented here does seem a lot like a Darwin-style devil's advocate leadup to an explanation of why bipedalism is useful, but without the context I just don't know.]

At 0:05:06 the statement "But it's still much faster to run on four legs" simply ignores how hominids probably dealt with predators, which is how chimpanzees typically deal with predators. Bottom line: not often by running away, but by bluff and aggression coupled with noticing them first. In fact, although chimps can be killed by leopards, tracking studies of leopards wearing radio collars show they tend to go out of their way to avoid chimpanzee groups, something also noticed by researchers who study chimps. More details in the "How chimpanzees react to predators" section on my [Predators](#) page.

Also -- and this is a trivia note really, since running away isn't likely to have been our ancestors' main predator avoidance strategy -- I've often seen it claimed that apes are faster runners than humans, but I've never ever seen it backed up with any data whatsoever. I wouldn't be surprised if it were true, but also wouldn't be surprised to find it just seems that way because those four legs moving about just seems so much busier. If anyone out there has any *actual data* about the relative running speeds of, say, chimpanzees and humans I would really like to hear about it. Please contact me with the feedback [mailto](#) link if you find any.

Now Peter Wheeler is presented giving a very good and easy to understand reason why bipedalism helps a great deal in a hot open environment -- simple physics. Since the ground heats up from the sun, it gets cooler the further up you get from the ground -- it's some 20 degrees centigrade cooler at head level than ground level for an upright hominid. But this gives only a small part of Wheeler's hypothesis, which covers lots of interesting ground and answers several of the

questions the AAT/H claims to be asking in good faith -- for instance about body hair, head hair, and sweat. That doesn't rate a mention here though.

On the general tactics front, when they mention Wheeler's hypothesis they do so as part of a section suggesting that each of these ideas is not part of a long list of good reasons for bipedality in hominids, but must instead replace any idea that came before ("one early idea was" as if it was ever dropped, then "or maybe..." followed by "Or even better..." which is in turn followed by "a more recent explanation..." and yet again "and there's another way..."). By doing this, they make it seem that there's only a single explanation, or competing suggestions, at any time, when there's a long list of good reasons that compliment and reinforce rather than compete with each other. This follows Morgan's long-standing tradition of apparently thinking that having only one single reason is better than having a bunch of reasons for doing something, which doesn't make sense. It also seems to be an attempt to forestall the obvious way that wading should be incorporated into possible reasons for bipedality, as part of a longish list; this is anathema to Morgan and her followers. For them it must be the only thing; all or nothing.

On the media critique/tactics front, notice how Peter Wheeler is introduced as "savanna physiologist Peter Wheeler" instead of just noting that he's an anthropologist who studies a range of topics in a range of areas. For instance he's co-written with Leslie Aiello a well-known and oft-cited paper on what they term "the expensive tissue hypothesis: the brain and the digestive system in human and primate evolution". They've also (since this documentary) studied Neanderthal climate adaptations, which is hardly the purview of someone stuck on the savanna. No one else is labeled in this narrow and misleading manner; is it a coincidence that he is also the only critic allowed to demonstrate any part of their own ideas, indeed anything but the vaguest of negative reactions? I don't think it's a coincidence. Wheeler's demonstration is clear and easy to understand, even though it's just a small part of his overall "Radiator Hypothesis". By labeling Wheeler in the way they do, the program compartmentalizes him as some sort of booster of only savanna-related ideas instead of someone who follows data where it takes him. Since the latter would be counter to the program's theme (in their scenario it's Morgan, and only Morgan and her supporters, who follow data where it goes) the accurate ways of labeling him are thrown out the window and a restrictive and inaccurate label is used instead.

Now the program starts in on summing up.

Phillip Tobias shows up demonstrating he hasn't actually looked at the stuff he's talking about in this program (sweat glands in this case). When he says "all these

things" (he actually is talking only about sweating, but names sweat glands and perspiration as if they were two completely unrelated things) "could indeed be valuable pointers to a kind of aquatic component to human evolution", it shows Tobias hasn't actually looked at those things, just heard Morgan's side. She, unfortunately for Tobias, doesn't report it accurately, and certainly not fully. When you look at them they don't point to "a kind of aquatic component".

The next section lists a few things and says "all these qualities contrast strongly with every other savanna animal". And yet we did indeed live there, even with those contrasts, even before we had modern technology. Doesn't that fact count at all? Not even the teeniest little bit? Not to AAT/H proponents, apparently, or to the producers of this documentary.

Besides, is their statement true? First, you really should include savanna primates, don't you think? They have much the same characteristics as we do, although since we have a more effective sweating apparatus I bet we do drink more water than they do -- although unlike the program, which takes its cues from Morgan, I don't pretend to know for sure, since I don't have the data. Some dry-adapted animals certainly produce very concentrated urine, unlike us, and at least some of the ungulates among them can shut down some of the blood flow to their brains via specialized arteries in their necks (they still die if they overheat; exploiting their overheating is how persistence hunting works), but unlike Morgan I am not shocked that we aren't like distantly related animals and instead resemble other primates, such as savanna-dwellers like baboons and patas monkeys, not to mention savanna-dwelling populations of chimpanzees (each time this comes up I am newly amazed, as if for the first time, at how AAT/H proponents and Morgan in particular are apparently *shocked* that we resemble our close relatives instead of distantly related animals like Morgan favorites "the wild ass and the camel"). And as I pointed out -- apparently this does need repeating -- we do and did live there, and even hotter, drier places, at a time when our species didn't use any of our present-day "high tech". So it's perverse to suggest that we couldn't do what we did in fact do for hundreds of thousands, indeed millions, of years. Yet that's what Morgan and the documentarians are suggesting here.

To correct another falsehood from this section of the program, before the 1990s australopithecines were known to have lived in wooded and forest habitat as well as more open savanna, plus this section again offers the strawman version of savanna as treeless.

Here's a good bit, the narrator says "the main counterargument to the aquatic ape theory, that humankind was born in the savanna, was losing support". First, this presents *once again* the strawman environmentally deterministic savanna theory rather than the theories actually used in paleoanthropology for over a quarter

century. An environmentally deterministic savanna theory was not "the main counterargument" to the AAT/H. In fact even the actual paleoanthropological theories used were not "the main counterargument" to the AAT/H. "The main counterargument" to the AAT/H was that the "facts" it used as evidence were mostly false, that it depended on misunderstandings of evolutionary theory, strawmen and a host of other logical fallacies.

This leads into Morgan demonstrating the use of a logical fallacy (false dichotomy) as she claims that "this was a tremendous boost for the aquatic theory because there is nothing else really taking its [savanna theory] place". Lee Berger then correctly points out that even if one scientific hypothesis is shown to be definitely false, it doesn't lend support to any other theory. This is something that people often don't seem to get, and Morgan has long used this non-understanding to her advantage (assuming she understands it herself). If you have two competing theories about what 2 plus 2 adds up to, and one says 7 and the other 9, proving that 2 plus 2 does not equal 7 does not lend the slightest weight to the claim that 2 plus 2 equals 9. It really wouldn't matter if Morgan was right when she says "the aquatic ape theory is the only game in town" (and she's not right); even if *every other theory in the history of the world* was shown to be wrong, it doesn't lend support to any theory still standing; each must stand on its own, and the AAT/H falls down rather badly even without comparing it to any other idea.

As the fifth segment of the program starts, they bring up the Internet with the statement that "the Internet put scientists all over the world into instant touch with each other, and the aquatic ape was one subject that could be relied upon to produce fierce debate". This is one of those true but false things; yes, it put scientists in touch with each other, but at that time newsgroups, which is what's being referred to in the second part of that sentence, had virtually no scientists posting in them. Even now newsgroups are generally a very poor place to try to run into a scientist. This is especially true of those dealing with anthropology and/or evolution, since they tend to be overrun with various forms of science-deniers and fringe and pseudoscience promoters.

At the time Elaine Morgan first went online there were very few scientists online in public forums; the group she mentions had one, anthropologist Ralph Holloway, several grad students, and amateurs like me and others. I love Morgan's mocking tone with her dismissive "all the little objections they had" statement and the claim that these were being "raised behind closed doors" and she was previously denied access to them.

As the TV crew follows Morgan to the Dual Congress in 1998, here's a wonderful passage at 0:02:00 promoting Morgan; with the narrator's tone and the visuals, it's like she's being beatified:

Narrator: "After all Elaine Morgan had written about Africa and its fossil sites, this was her first visit to either. She went on a conference day trip to see first hand the labors of the fossil hunters. She touched the bones of our past, and discovered how much the rest of the world knew of her ideas."

Unidentified guy at site: "I think there is, ah, a lady who insists that the ancestry of humans was in water."

Elaine: "Yes, that's me"

Unidentified guy at site: "You!"

Love that awe-filled "she touched the bones of our past". And notice how the editing answers the question of "how much the rest of the world knew of her ideas". Not much, apparently. No wonder people don't accept her ideas; just look at this guy they show! Ah, the beauty of film editing.

Narrator: "And then, after 30 years of studying the evidence from afar, Elaine Morgan now walked in the footsteps of the celebrated fossil hunters."

The awe-filled tone again. Beatification complete!

The program now gets into the nutrition and brain brigade:

Narrator: "The only other mammals [visuals: clip of bottlenose dolphin] with brains as large and sophisticated as ours are marine mammals. What does that say about the marine mammals' diet? Do you need a large brain to exploit sea food? Or does sea food give you a large brain?"

Good question... which has been answered. First, on the tactics front, notice that they've offered a false dichotomy of only two choices as to why we see the patterns of brain size we do. Neither is correct. A look at marine mammals' brain sizes shows a range that fits not a "marine" diet vs. terrestrial diet, but whether they are social or not, and whether they are predators or not. Just like terrestrial animals. Some species of dolphins, like the bottlenose dolphins pictured, have pretty large brains, others don't (those which do are social predators). And other marine mammals which have that same marine diet that supposedly "give[s] you a large brain" --

such as seals -- don't have particularly large brains except when compared to non-predators and non-primates (most primates have relatively large brains).

Look at that passage again as a study in tactics. The first sentence suggests that all or some large number of marine mammals have brains as large and sophisticated as ours, when the truth is that only one or two species approach (but don't match) ours. Most are just about where we would expect them to be from studies of brain size of predators versus prey and social mammals versus non-social. There's more about this on my short page on [EQ \(Encephalization Quotient\)](#) which I did when critiquing the BBC Radio 4 aquatic ape show.

Now note that in all this discussion of brain size the time period has shifted radically, without mentioning it, from the ape-human transition to millions of years later and the transition from australopithecines to *Homo*. With this all the Morgan arguments about bipedality fly out the window, yet this is not even hinted at in the documentary. Her other supposedly (but not actually) "aquatic" features also no longer fit the time period discussed. In fact with this time shift her entire theory -- dependent on the time period of the transition -- is thrown bodily right out the window, *but this defenestration receives no notice whatever in this documentary*.

Also, even gathering mussels and catching fish, how does it give us the features of seals and whales? When Leigh Broadhurst mentions the easy to catch fish at the end of the rainy season, when they're stranded -- much as almost every animal in the northern part of the northern hemisphere eats salmon during and after their run -- how aquatic are you being when you walk onto a mud flat and pick up a fish? The program answers that one:

Narrator: "So early humans were somehow intimate with water. They either used the life in it or were part of the life in it."

Could they be any more vague? This incredible vagueness about what exactly they're talking about (eating a shrimp cocktail apparently makes you aquatic to them) is a long-standing part of the AAT/H.

This may be my favorite part of the program:

Narrator: "As time has passed, Elaine Morgan has been vindicated. She hasn't been proved right. She isn't even supported by a majority of anthropologists. But she's recognized, and her ideas are sparking other ideas."

Recognition itself as vindication is a modern thing all right. Being a celebrity, apparently, is what "vindication" now means. Not being right, not being supported

by science, just mere recognition. And of course, her ideas are sparking ideas that contradict hers, ideas that are poorly supported, etc. Spare me from that kind of "vindication".

Phillip Tobias shows up again: "I see Elaine Morgan, through her series of superbly written books, presenting a challenge to the scientists. To take an interest in this thing, to look at the evidence dispassionately, not to avert your gaze as though it were something that you hadn't ought to hear about or hadn't ought to see. And those who are honest with themselves are going to dispassionately examine the evidence. We've got to if we are going to be true to our calling as scientists. "

Very noble. Of course Tobias here incorrectly assumes this wasn't done. What exactly are you supposed to do if you look at an idea and immediately see it's full of huge holes? Spend lots of time, energy, and money pursuing it? Is not spending that time, energy, and money on a badly thought out and presented and supported idea really not being "dispassionate"?

Desmond Morris tells us that while "Elaine Morgan and certain others have sought for every scrap of evidence they can find to support the aquatic theory", "some of the more conservative anthropologists have searched for every scrap of information they can find to destroy that theory". Name one. Come on, the producers can't even be consistent; one second anthropologists are "ignoring" it and "averting their gaze" and literally two seconds later anthropologists are hard at work searching diligently for any scrap of contrary info. Make up your minds, guys.

Morris says that "instead of having a balanced judgment, what you have is a counsel of the defense and a counsel of the prosecution" but in fact it's just Morgan who, as she has admitted, argues like a lawyer rather than a scientist, for instance burying any contrary information she finds. This method has become so ingrained to the AAT/H that when I looked for data on hair and swimming speeds, found conflicting data, and therefore wrote up and posted the data both for and against the idea that reducing body hair would improve swimming speed, at least one prominent AAT/H supporter thought I must have done so by mistake (see my [Algis Kuliukas page](#) for details).

The program closes with the statement "did humanity rise out of water? It has to be considered."

Done; just because Elaine Morgan doesn't like the inevitable answer you get when you consider it doesn't mean it hasn't been done.

Review/Critique of Elaine Morgan's 2008 book *The Naked Darwinist*

"I don't have enough new material to fill a sixth book on the AAT (aquatic ape theory). Instead, this is an attempt to provide an on-line introduction to the idea for those unfamiliar with it, a brief account of my part in promoting it, and the reception accorded to it, and the current state of play."

Elaine Morgan's website (www.elainemorgan.me.uk) 27 July 2008

"Take plunge for 'aquatic ape' theory latest"

May 1 2008 by Our Correspondent, South Wales Echo

BAFTA award- winning playwright Elaine Morgan is set to launch her latest book on a controversial view of human evolution.

Best known for almost single-handedly developing and championing the theory that humans are descended from an aquatic ape ancestor, the author will be visiting Cardiff University on Tuesday to talk about her new book, *The Naked Darwinist*.

...

The book launch will take place in Committee Rooms One and Two in the Glamorgan Building on Tuesday.

Elaine Morgan launched a eponymous web site a couple years ago, and prominently displayed the above first quote. But apparently a whole lot of new material came available in a hurry, since she found herself able to fill 94 pages of that new sixth book with it in short order, short enough, in fact, to have the book launch several months before she got around to taking down her quote about not having enough new material from her website. So naturally I'm eager to see just what this new info is and how she's answered the many valid objections to her ideas.

Yikes, the very first section, entitled "Don't ask", launches right into "lost our body hair" and "hairlessness". GIGO.

I hate to belabor this, but if you are asking how we (or any other organism) came to have the characteristics it does you had better get the description of those characteristics correct, or you have GIGO (garbage in, garbage out). If you're just having a barroom conversation I wouldn't object overly to this being called hairlessness, even though that's very inaccurate (although you'd better watch out

for barroom wags making a bet with you when they spot easy money. :) However, Morgan isn't having a barroom conversation; she's trying to do science, and when you do science you'd better get your descriptions right or you haven't got a hope of getting a correct answer.

Comparing our hair characteristics accurately to our near relatives what you find is that among primates there is a progression in number of body hairs, with apes and humans having fewer than monkeys, and the difference between humans and chimpanzees is not so much number of body hairs, but length and thickness, a difference caused by relatively minor genetic change, so minor we see almost as much variation among people today as between chimps and humans. So we have shorter and finer body hair, longer head hair, tufts of hair around the genitalia and armpits (and for that matter a great many humans have quite a lot of body hair overall). This makes us different from apes, but there's another group of mammals we resemble even less, when you look at these hair characteristics accurately: there are no aquatic and semi-aquatic mammal whatsoever sharing those characteristics. None of them do. Not one, anywhere.

Uh-oh, page 5 and we're back to another Morgan classic: why aren't we like camels? Yes, AAT/H proponents constantly ask, why are we so much like our primate relatives? Why, oh why? They get very confused about why on earth this would possibly be so. The rest of the world, outside of some science-deniers, accepted the answer to this at least a hundred and fifty years ago (the answer is phylogeny, relatedness, the central idea behind accepting evolution) but AAT/H proponents like Morgan apparently still find it baffling.

Morgan then asks *"Why did our ancestors so often respond to exactly the same problems that confronted other animals by adopting diametrically different strategies?"*

Oh I don't know: because this is what we see in animal after animal? why lions and leopards met the challenges facing them by lions being more social and leopards less so? why skunks evolved extremely pungent scent glands while porcupines met essentially the same predator challenge with quills, and pangolins did it with body armor? This variation in responses to common problems is the norm, but Morgan has always seemed to have a difficult time understanding that it is normal for this to be the case. It's also common, mind you, for people to think of ourselves as uniquely different. But this is an artifact of our own navel-gazing, our intense interest in human beings over interest in other animals. Part of this is practical. We've biased our research toward humans and what makes them tick, so we have a lot more info on humans than on most other animals. Research money and time -- always limited -- was considered better spent on the affairs of humans, and a lot of our info about other animals is less a result of interest in them per se than an

interest in what their physiology can tell us about us, or how we can better raise them for food or workers or pets, or medical research subjects.

Only relatively recently -- partly as a result of technology getting cheaper, partly because older, often easier, areas of research are occupied and new researchers want to stake out their own territory -- are we delving into the minutia of behavior and physiology of non-human animals to anything even remotely approaching the degree we've done with humans (it still isn't close). So we tend to find it easy to see ourselves as special, while when we look at virtually any animal on earth in detail, we find they are all also special and different from even most of their near relatives -- in fact that's why we can divide them up into different species. But a very superficial look and very superficial knowledge obscures this, which again, may be forgiven in a barroom conversation but not in science.

Morgan, page 6: *"A recent weblog on pseudoscience posed the pertinent question: 'If the Aquatic Ape Theory explains so much, why do the majority of anthropologists not subscribe to it?'"*

Asked -- and answered -- by John Hawks, in his [25 January 2005 blog post](#), as he explains why even a cursory thought -- even *without* a detailed analysis -- leads reasonable people to reject the AAT/H. Morgan, of course, ignores his answer with a wave of the imperial hand, never addressing it. Hawks allows the AAT/H claims to go unchallenged, even though they don't stand up to scrutiny, but shows that even if those claims were unchallenged the idea has a gigantic gaping hole: since the aquatic ape does come back to a terrestrial environment -- including the hated-by-AAT/H-proponents savanna -- it requires *two* changes instead of one for a number of features, and requires some bizarre selection pressure to keep these "aquatic" traits intact in a terrestrial animal, in an environment where AAT/H proponents like Morgan say these features could not evolve. This throws the AAT/H's claim of being parsimonious (one of their faves) out the window.

Of course some of the "new" proponents, like Verhaegen, simply claim that humans always have been semi-aquatic right up until recent prehistory, ignoring masses of evidence that this is not so, ignoring fossil sites, etc. No wonder even participants in a recent "aquatic ape" meeting found the reasoning on some of his ideas "idiosyncratic".

She gets into the nitty gritty of her story with Raymond Dart, but in her enthusiasm suggests, rather strongly, that Dart's discovery of the Taung child started hominid fossil hunting in earnest, and began the time when fame was attached to hominid fossil finding. The reality is of course that both those aspects had started -- in

earnest -- with the much earlier finding of Neanderthals in Europe. However, it's certainly true that Dart's discovery, and the acceptance of it as a hominid, kickstarted the hunt for earlier hominids, what we now call australopithecines, simply because until then it wasn't actually known there were such things. Even *Homo erectus* had previously had difficulty being accepted as an early human ancestor since it was thought at the time (nearly a hundred years ago) that over a millions years was just too early for hominids, and it was generally assumed that the earliest "human" trait would be a more or less human-sized brain (that earliest trait -- from what we know so far -- turns out to be bipedalism).

Morgan, on page 9, starts to make a good point about the effect of specialization on studies of human evolution, but she immediately goofs there too, because she doesn't seem to understand that one big problem with looking at things that don't fossilize, which historically is most or all of what the AAT/H has been built on, you need to be able to test and investigate your hypotheses if you want them to be more than idle speculation... you know, like barroom conversations.

We haven't had the capability to do this until pretty recently, although some aspects can be demonstrated, like Peter Wheeler's "radiator hypothesis" -- briefly, very briefly, Wheeler has done a series of papers describing his studies of how shorter body hair arranged as human body hair is arranged, along with human style sweating, is an immense aid in cooling in a hot, dry, relatively open area, allowing the use of that environment at times when other animals find it difficult, or even effectively impossible, to utilize it. So Wheeler, using accurate descriptions of our body hair and sweating abilities, was able to show how our hair and sweat characteristics would be an advantage in dry hot areas, and observations from cultural anthropologists show how this was indeed effective, for instance in persistence hunting -- also very briefly, "chasing antelope or other game in the midday heat, often for hours, until the animals overheat and collapse" (quote from *Natural History* magazine); the key here is that our eccrine sweat glands can keep flowing and therefore cooling us while the prey's apocrine sweat glands -- which Morgan has long held are superior to ours -- also work fine, but only for about 20 minutes and then need a recharging period, at which point the animal heats up and has to slow down or die while we keep going.

By the way, the knowledge about persistence hunting is an example of the other big problem I've seen over the years resulting from specialization, and from what I've seen it's a far bigger and more common problem than stones and bones people not looking at what anatomists are doing. They're far more likely to look at the work of anatomists, and for that matter to be trained anatomists themselves, than they are to look at the work of cultural anthropologists. This has led them to error at times, but I must say that the team approach taken to fossil interpretation in the past 25

years or so has helped that immensely. So has the rise of journals like *Journal of Human Evolution*, which has papers not only from stones and bones people, but also primatologists and cultural anthropologists. This was thought by many to be an oddly unfocused approach when *Journal of Human Evolution* first came out, but once they saw how helpful it was minds changed, which is what you see over and over in science, any science.

On page 13 Morgan mentions Max Westenhöfer, which has become common among AAT/H proponents. Less common -- well, virtually non-existent it seems -- is looking at why Westenhöfer's idea never got any traction. From elsewhere on my site: In recent years it's become popular to point to a German-language publication on an aquatic hominid past from well before Hardy's, by German anatomist Max Westenhöfer. As a note of historical curiosity this could be sensible, but there seems to be a thought that it somehow legitimizes the idea of the AAT/H. However, there is no indication that Westenhöfer's "aquatic human" work influenced anyone within the AAT/H community, much less outside it. This should not be surprising, since from the excerpts I've seen translated it was really chock full of wildly mistaken notions, and concludes, in the words of one translator (Patrick Beck), that *Homo sapiens* cannot be a close relative of primates, and even uses fiction as evidence, as in the statement "For example, to me the story of Beowulf's struggle with the dragon under water is a hint that humans perhaps did live and fight with such dragons in water". This, to me, is not someone I'd like to claim is on my side :).

Page 14: Le Gros Clark supposedly phoning Alister Hardy in 1960 and telling him to stop talking about the AAT. Ever really happen? any evidence? No evidence has ever given for this bizarrely unlikely claim. AAT/H proponent Algis Kuliukas had, at the time he said it online, also falsely claimed that Le Gros Clark had been defending Piltdown Man up until a few months before Hardy's talk in 1960, while in the land we like to call reality Le Gros Clark had actually co-authored the definitive debunking study and report on Piltdown Man in 1953. One can only assume that the tale of Le Gros Clark admonishing Hardy is of similar veracity.

Morgan continues her expose of The Great Anthropology Conspiracy on page 14: *"The whole affair was quickly smothered. Alister reserved the right to contribute one article to The New Scientist and give one talk on the BBC's Third Programme, to correct some garbled versions of what he had actually said. His colleagues then forgave him, comparatively few people ever heard of his gaffe, and for the time being the matter was closed. After his death, when a memorial service was held to honour his memory, no mention was made of this incident in his life, as if it had been a discreditable aberration on his part which it was kinder to forget."*

Any evidence that *this* conspiracy ever happened?

What about the evidence that it didn't? For instance, the fact that Hardy wrote several articles about the idea, not the one Morgan claims he "reserved the right to" (apparently at the Great Anthropology Conspiracy World Headquarters). And she's certainly aware he did so, since she reprinted one of them in her 1982 book. Now it's true that at a memorial service it was perhaps "kinder to forget" the poor quality reasoning that Hardy put into this particular idea (more details on my [page about Hardy](#)), just as it was probably kinder to downplay his interest in telepathy and psychic experiences which he spent quite a lot more time and energy on than he did the AAT/H, and for which he set up a trust for research. After all, his actual marine biology work was terrific and extensive, and isn't it nicer to talk about the deceased's good points than his failings? (That, of course, is what my mom always told me; was my mother wrong about that?)

The next chapter, pages 16-19, are a rant about why on earth professional anthropologists didn't do extensive critiques of her first book, a pop book which *by her own admission* was poorly researched (she even puts the word "research" in quotes when describing her own research as "sketchy and superficial"). Another word, hubris, comes to mind.

Page 21-22: *"I have never bought a book about spiritualism or UFOs in order to weigh up carefully the arguments for and against. I feel convinced that they must be tosh, just as others were convinced that mine must be tosh."*

Oh dear, poor Alister Hardy would be so disappointed to have the major work of his last 20 years (and which he had been thinking a lot about for the last half century of his life and set up a trust to continue after his death) dismissed so offhandedly.

On page 22 Morgan discusses her 1994 online debut: *"When I joined a group discussing evolution, I was met with a torrent of scorn and hostility and urged to get out of their air-space and go back where I came from."*

Oh dear again. Having been there and seen Elaine Morgan's debut online and the reaction to her, she's --- how should we put this? -- not being entirely accurate. She was treated, for the most part, respectfully, although certain people, me included, spent a lot of time pointing out her many mistakes and misunderstandings of both fact and theory. I know that is never fun, but it is, as Carl Sagan pointed out, doing you a favor. A lot of people spent quite a lot of time doing Morgan favors which she seemingly does not appreciate in the least. She also doesn't seem to remember her own insults and general snideness -- she generally alternated between that, or a sort of unctuous attempt at flattering and cozying up to people, or a pose as a somewhat imperious "voice of authority" -- sometimes she'd try all of them aimed at one person at different times.

An abbreviated timeline:

13 Dec 1994

Morgan makes her first online newsgroup post

7 Jan 1995

"Hi Phil (Chris?) This is fun, isn't it?"

My, she's awfully nice about being met with a "torrent of scorn and hostility"; what a mensch.

13 Jan 1995

"Science papers. Oddly enough I have had science papers published. I've even had two solicited. I've had a letter (presumably peer reviewed) published in Nature. Not oddly at all, none of these contributions had anything to do with AAT."

Morgan here betrays the fact that after more than *two decades* of writing about science, she didn't understand peer review in even the slightest, most superficial offhand way; letters to Nature, like letters to any journal I've heard of, are not peer reviewed. I have no idea what "had science papers published" refers to; I've never heard of them.

24 Apr 1995

"First to all critics of my "ranting and raving" paper some time ago: OK, point taken. apologies, promises not to do it again etc. "

I thought *everybody else* was ranting and raving.

11 Apr 1996

"Strange how these resemblances occur to people. Has anyone ever told you how much your style resembles Joe McCarthy's?"

Very nice.

21 Apr 1996

"You are just smearing like the worst kinds of politicians."

Very nice again; remind me who it was who spewed "scorn and hostility"?

She also had a habit of creatively "quoting" newsgroup participants (ie. rewording their comments and placing the result in quote marks as if it actually was what they'd written) and even, on one occasion at least, trying to make others look bad by accusing them of having agreed to these creatively "enhanced" quotes.

Page 23 has more of the GIGO problem which inevitably results from imagining humans to have hairless bodies rather than describing just what our hair characteristics really are (instead of as Peter Wheeler, for instance, has always done accurately in his hypothesis about a function of hominid body hair).

Page 24 has the return to Morgan's writings of Leon P. La Lumiere, whose idea -- the Danakil Alps -- she hasn't been writing about for quite some years. Morgan claims Leon P. La Lumiere was a geologist but he's described in *The Aquatic Ape: Fact or Fiction?* as an acoustic physicist; he worked at the US Naval Research Laboratory, and the only non-AAT/H paper of his I find was indeed on acoustical engineering. (Note that Morgan was familiar with La Lumiere, reprinted his paper in her 1982 book and was heavily involved in the conference that led to the 1991 *The Aquatic Ape: Fact or Fiction?*, even so far as rewriting and combining La Lumiere's papers into one article for that book; it seems unlikely she would be unaware that he was not a geologist.) He disappears within a paragraph however, which is probably wise. Morgan used to use La Lumiere's idea to claim that a group of increasingly aquatic apes spent several million years stuck on an offshore island unable to leave -- that always did seem odd; no wonder she dropped it. This time out Morgan seems to have a bit of a time travel problem with La Lumiere, claiming that up until La Lumiere wrote up his idea in 1981 the oldest hominid fossils had been found in South Africa, and that La Lumiere therefore pointed out a valuable new hominid fossil area. But of course Donald Johanson took over the Afar fossil site well before that (Lucy was discovered in 1974 and that was the 3rd year Johanson had been looking for hominid fossils there) and the Leakeys had been finding what were then very early hominids in Kenya for 20 years before that. The idea Morgan here promotes, that La Lumiere pointed a fossil area not already well known, is nonsense.

Page 24: *"I am no longer wedded to the idea that the immersion was in salt water rather than fresh - it could have been either."*

She still hasn't learned what our sweat, tears, and general physiological behavior toward salt tells us about that. It rules out -- about as categorically as possible -- that it *just could not be* in salt water. More details in my [sections](#) on [salt](#) on this site. This has been pointed out in *excruciating* detail to Morgan for nearly 15 years and she still -- based on this sentence -- doesn't understand it.

Page 24 has the appearance of Marc Verhaegen. Morgan does have my sympathy on the matter of her supporters. Lots on why I say this in my [page on Verhaegen](#).

On page 26 she mentions me briefly only, apparently, to suggest that all I do is personally attack her; you'll have to judge that for yourself. Let me just point out that her doing this is a tactic, in logical fallacies it's usually referred to as poisoning

the well. Then she pointedly refers to acceptance of "the" (there is no one "the" theory in human evolution, except for the obvious one that we did evolve and are most closely related to African apes) "revealed truth", which is part of her ongoing tactic of claiming that opposition to her ideas are merely closed-minded, brought about, apparently, by a religious-like adherence to "the revealed truth" that frankly doesn't resemble any aspect of any science I'm familiar with. But then I've never even been able to find the headquarters of The Great Anthropology Conspiracy or get invited to any of their meetings; seemingly I'd have more luck becoming a member of The Illuminati.

On page 28 Morgan does some nifty writing in which she manages to complain about being ignored by academia via the technique of repeated invites to present her ideas in universities. Very impressive.

On page 30 she talks about the 1987 Valkenburg (*The Aquatic Ape: Fact or Fiction?*) conference and says she learned there about hymens not being an "aquatic" trait and dropped it after. True? Well, no.

First, Morgan used it in her 1990 book *The Scars of Evolution* and later admitted online that she had. At that time she said she dropped it after *that*. She did mention that -- as well as toothed whales, seals, and dugongs (she forgot manatees for some reason) -- guinea pigs and lemurs have hymens, but despite admitting these non-aquatic animals have hymens she still bizarrely claimed that "an aquatic connection seems the likeliest explanation. She also neglected to mention other animals with hymens: llamas, horses, hyenas, elephants, rats, and some species of galagos.

But Morgan has apparently also forgotten her 1997 book (page 151, mentioned on my [page](#) critiquing that book), as well as many online mentions over the years. Now Morgan uses a common tactic of hers on this subject (and she's used it over the past decade on things like tears and sweat as well); this is taking something she previously used as "evidence" and saying she's "dropping" it as evidence but continually bringing it up with a "I wondered, and still wonder" type of insinuation that it's somehow tied up with aquaticness, but carefully wording it so she can act offended if someone points out that she's still using it (there's an example of this method later in this book, and this critique, when she refers to ectoparasites).

She gets to John Langdon's 1997 *Journal of Human Evolution* paper, and doesn't like it, which is no surprise, but she really seems *offended* that someone did what she claimed to be asking people to do for years, to examine and weigh her ideas. She also either doesn't understand or deliberately confuses the issue of what Langdon refers to when he says (as she quotes): "*Heterodox ideas, he told us, feed*

on "a suspicion of and rebellion against established science and other authority", and have a special appeal for "peripheral segments" of society."

A big part of her (and some other) presentations of the AAT/H has been railing against a supposedly close-minded scientific establishment which means Langdon's application of the term heterodox ideas and his description of them fits Morgan's work perfectly. Morgan goes into an extended rant about politics of left and right and says these categories don't correlate with reaction to the AAT/H, which is both true and completely missing the point, if indeed she really missed the point and isn't just using a tactic to try to get *you* to miss the point of Langdon's remark.

Interesting that when Morgan mentions Langdon's examples of "peripheral segments of the population" she says *"He cites African Americans, and AIDS victims"*, which has an omission. Langdon had given some examples of heterodox ideas, theories not supported by the science but widely accepted anyway. He said "For some of the examples above the attraction is to AIDS victims, African Americans, and fundamentalists" and that Morgan left out fundamentalists is interesting -- maybe just to me -- for a couple of reasons. One is that she obviously wants to be lumped in with only two of those, the ones people are more likely to be sympathetic to; the other is that -- sadly -- her methods are very similar to those of fundamentalist creationists -- altering quotes or taking them out of context, selectively either using or hiding data and twisting data to claim it says something it doesn't.

On page 37 Morgan uses a time-honored technique of hers of prominently mentioning of hominid fossil sites *"In many cases they were accompanied by the remains of fish and crabs and turtles and crocodiles and the odd hippopotamus"*, but now also gives an offhand *"but also by occasional land animals"*. Although she doesn't name Lucy this time that fossil is what she's referring to here. Of course this doesn't accurately describe the situation of many fossilized hominids, but it's an improvement over her previous tactic of not mentioning the many -- not "occasional" -- terrestrial animals at all. And with Morgan, one learns over the years, one must be content with small steps. Very small steps.

She mentions taphonomic bias but doesn't actually describe it well; it seems to me she does so as a tactic to mislead the reader but I could certainly be wrong about that; however, that would mean she simply doesn't understand it after repeated explanations and decades of study. Which isn't better, really.

Taphonomy is the study of how things fossilize, where they do, and how to tell something about numbers, habitat, etc. from the number and type of fossils we find. For instance, very small mammals tend to be underrepresented in the fossil record because their bones get crushed too easily. Which bones of a skeleton are

present or missing can tell us whether predators and/or scavengers were involved, and often what type. Also animals which live in water or along shorelines tend to be overrepresented because they live in places where they are likely to get fossilized. In fact taphonomy is one of the biggest marks against the AAT/H since we would expect such a creature to leave an enormous number of fossils and we don't find this for hominids, especially early hominids.

On pages 37-38 Morgan attempts to gloss over the fact that fossil evidence doesn't show any signs of a semi-aquatic past for hominids in what has become a classic manner; she makes the otter/polecat skeleton claim again:

"It would be just as problematic to dig up the fossil of a mustelid and ask a palaeontologist to determine whether its life-style had been that of an otter or a polecat."

This is something Morgan has been saying for years now and it's just silly. What she's doing is a logical fallacy called argument from incredulity, basically that if she can't imagine being able to do it, it can't be done. I'm sure Morgan couldn't do what she suggests, and neither could I. The difference between us is that I know perfectly well that I'm not a trained anatomist and that such people can do a great many things with bone and fossil identification that I cannot. For instance, can you glance at a fossil tooth and tell what kind of animal it's from? For most of you the answer would be no, just as it is for me, but for many scientists this is a trivial exercise. If a paleontologist could not do what Morgan says they can't, why this paragraph from a [discussion of fossil otters](#)?

"Otters are distinguished from other mustelids by the modification of their skeleton for swimming. A nearly complete skeleton of Satherium collected by the Smithsonian shows that, like living otters, it was well adapted for life in the water. In many swimming mammals, including the otter, the femur (upper leg bone) is short and stout and the tibia (larger of the two lower leg bones) is long. The tail is long and muscular and along with the hind legs helps to propel the animal through the water. The skeleton of Satherium has all of the features one would expect to see in a strong swimmer. These features, along with the structure of the skull and teeth, support our interpretation that Satherium like the modern river otter could maneuver in the water to catch fish, frogs and crayfish or uncover freshwater clams hidden in the mud."

It's foolish to imagine that just because you can't do something yourself no one else can, and foolish to make a statement about what experts can't do without checking, and as with other examples on this page, this required only the teeniest bit of research in the form of a short online search. That Morgan can't be bothered to do

even this teeny bit of easy research and yet expects science to fall at her feet to accept her conclusions speaks of a massive ego.

Starting on page 38 she gets into a false definition (along with false dichotomy) of savanna versus areas with trees, etc. then gets indignant: *"One reaction caught on film was 'Just because the Savannah Theory is wrong, that doesn't mean the Aquatic Theory is right'."*

What is it with Morgan's tendency to write something other than what a person says yet put it in quotes? Is it because she's won an award for docudrama, in which you make up conversations and put them in the mouths of historical figures as if they'd actually said them? Lee Berger, in the 1998 BBC documentary Morgan was involved with, actually said "If one testable scientific hypothesis like the savanna hypothesis is out the window, because we found provable evidence that these apemen lived in forested environments, that doesn't have any relationship to the aquatic ape hypothesis". It's difficult to imagine why Morgan finds this sentence so offensive, since it's obviously true. Even if there were a "savanna theory" in anything like the sense that Morgan claims (ie. an environmentally deterministic theory) and the only other theory out there was the AAT/H (which isn't true either), proving one wrong certainly *does not prove the other right*. It doesn't even lend it any support. If you have two competing theories about what 2 plus 2 adds up to, and one says 7 and the other 9, proving that 2 plus 2 does not equal 7 does not lend the slightest weight to the claim that 2 plus 2 equals 9. Of course the actual theories of human evolution from the last 30-40 years (at least) deal heavily with food, food-getting and sharing, and social interaction, and only tangentially with environment; they are not and were not environmentally deterministic as is Morgan's own AAT/H and her strawman "Savanna Theory".

On page 40 Morgan says *"But some of that fossilized pollen was of lianas. You do not find lianas in parklands."*

This is part of a confused passage where Morgan advances her usual false dichotomy of forest versus "open grassland" (sometimes it's "dry, arid plains") to advance her strawman version of savanna, ignoring what savanna actually means in science. (I'm forced to assume that by "parklands" Morgan means "savannas" otherwise the sentence would be some bizarre non sequitur.) The interesting aspect of this particular sentence is that it demonstrates how Morgan simply uses her own assumptions rather than actual data. Lianas do grow in forests and are often thought of as "jungle" plants, partly because of the image of Tarzan swinging through the jungle on a vine, but they also grow on savannas (and in forests actually -- counter to common assumptions -- tend to become more widespread in drier conditions). It's not that this means the lianas Morgan refers to were savanna lianas -- it's clear that early hominids were found in forests and then later wooded

savanna areas -- but the point here is that Morgan couldn't make the statement she does if she'd spent any time at all looking at the reality of lianas. For instance, with -- literally -- 5 minutes of online searching I found out about savanna lianas, including the [abstract](#) of an interesting sounding paper about a liana in West-African savannas whose thin sap is used as drinking water by local peoples. Morgan, on the other hand, obviously didn't do even this tiny bit of research yet felt confident about making her pronouncement, perhaps based on extensive studies of old Tarzan movies. So she just substituted her own dataless assumptions for reality. This cannot be done in any science you want to stand up under scrutiny.

Page 43: *"a small group of believers in the Aquatic Theory was invited to attend the Dual Congress which assembled hundreds of palaeontologists and biologists from all parts of the world in South Africa in 1998. So what happened next? Not a lot. Reviewers had some kind words for its entertainment value as a good read - "an engaging presentation" - but felt it obligatory to wind up with a curt phrase damning it. The Nature piece did it with "Only Morgan acolytes will warm to this appeal." The Sunday Telegraph was even more laconic. "What a pity it is probably bunk."*

The word "acolytes" certainly seems fitting insofar as it's reminiscent of the followers of Ayn Rand. (Thankfully Morgan's acolytes don't have positions of major influence over our financial institutions like Rand's do.) At any rate, the "only Morgan acolytes" line seems to be from Ian Tattersal's review of Morgan's 1997 book in *Nature* (14 Aug 1997) rather than some review of the Dual Congress.

I have seen a [writeup of the Dual Congress](#) online; the part about Morgan's presence mentioned the TV crew she had following her, which she's perhaps too modest to mention in her book:

The TV program's thesis was that science is a cosy male clique that has systematically and ruthlessly given the "cold shoulder" to Ms. Morgan and her hypothesis, primarily because she is a woman, and, secondarily, because she is outside conventional academia. Those who have met Elaine Morgan will know that she is a tiny doughty sparrow of a lady whose advanced years have not dimmed her, or her supporters' abilities to literally pin you against the wall so that she, or they, can make their point. Any refutation to Morgan comes less from an "anti-Morgan" conspiracy than simply from a strategy for survival! At the meeting, the session that Elaine Morgan was to speak at was quite ruthlessly stage-managed with "planted" questioners whose interventions would ensure that the conspiracy thesis would be supported by the session itself.

Bernard Wood

Having watched the BBC documentary Wood mentions ([critiqued here](#)) and noted the way it presents Morgan's preferred embattled housewife versus closed-minded establishment storyline (sometimes to a laughable degree), I find Wood's above description entirely believable.

Page 44: *"There are more papers than ever dealing with "When?" and "How?" questions, but the question "Why?" - the essentially Darwinian question - is silently by-passed."*

I would say (and have said online) that "why" questions are not really evolutionary questions, but this disagreement with Morgan's above statement may be more semantic than actual. Hard to say, since I have the feeling her statement is more a tactic than anything else. In evolution the question is not so much why, but why not, as Gregory Bateson pointed out many years ago. Evolution works by random mutation followed by selection; basically anything is "allowed" as long as it works well enough, so "why not?" is the question evolution "asks". As for specifically "Darwinian", I would say the essential "Darwinian" question is "how" -- how could life evolve as it has without invoking supernatural causes.

Page 44: *"Or sometimes not even silently. A paper published in 2007 with the intriguing title of A new model for the Origin of Bipedality laid this new policy on the line, pointing out that we are no nearer to agreeing on what benefits were gained by walking on two legs, but we have gathered a lot of new data about genetics. It proposes wasting less time on questions we cannot answer. "Perhaps we need to stop wondering about selective pressures and consider what kind of mutation might be involved ...*

"Stop wondering about selective pressures" is a codephrase for abandoning Darwinism, since selective pressures are the essence of natural selection."

She suggests here, none too subtly, that paleoanthropology has "abandoned" Darwinism, has been "Turning away from Darwinism", which leaves, it seems, only brave embattled AAT/Hers like Morgan to champion Darwin and evolutionary theory, I guess. Lucky we have such stalwart defenders of science to keep those candles lit in the long darkness the abandonment of science by paleoanthropology will create.

Notice here, as a study of debating tactics in Morgan's work, that she's here advancing her long-used method of suggesting, as unsubtly as usual, that academia works by edicts handed down from on high, orders from higherups which are obeyed. Well, in reality, what happens in science is that people don't hand down

edicts very often, because if they try they get laughed at, plus the edict would be ignored since the best way to get ahead in science is to point out how current thinking on some aspect of your subject is wrong and you're right; overturning a large amount of current thinking is surefire academic stardom. This reality doesn't fit with Morgan's long-promoted idea of a brave and freethinking outsider battling the rigidly controlled establishment, so in Morgan's writings reality gets thrown overboard. (She also tends to "disappear" the important work of women in paleoanthropology since the 1960s, work which changed the field, since this reality wouldn't allow her to claim, as she often does, that opposition to her ideas is largely fueled by opposition to women.)

There are a couple other problems with Morgan's view of science in this particular case. One is that it makes perfect sense to not spend much time, energy, and money on questions that can't be answered and concentrate on questions that *can* be answered... unless you're in a spirited barroom conversation of the "who would win in a fight, Batman or Godzilla?" type (the correct answer is "Batman". It's always Batman.) Another is that natural selection is not the be all and end all of evolution; even Darwin knew sexual selection was another important component, and in recent decades it's been recognized that there are other critical possibilities, such as genetic drift. Interestingly, Morgan has virtually always insisted that natural selection and nothing but natural selection be considered for any feature; she's long been solidly against even any invocation of sexual selection, probably because many of the features she seeks to explain via natural selection -- hair, sweat, fat -- are classic cases of sexual selection.

In the Bowers paper she mentions, two things: one is while it's already ludicrous to hold Morgan's idea that anthropology works via edicts from on high from eminent seniors the likes of Le Gros Clark, it's even more ludicrous that these supposed edicts come from relatively junior people in the field like an Associate Professor from Ball State University, no matter how talented. The other is that Bowers is pointing out that the change to effective bipedalism just isn't so difficult or drawnout a change as we once thought; it's not unlike changes to hair, where large physical changes come from minor genetic changes. In the case of bipedalism, all that's needed for a big physical change is fairly minor changes in a few genes which regulate development. This is part of a field which is big right now, the evolution of development, or evo-devo as it's shortened to. Although it's a "new" field, in the sense that it can be called a field, people like Richard Lewontin have been going on about this sort of thing for decades, and Gregory Bateson, famous for his work with Margaret Mead, suggested this aspect of development in evolution as critical many years ago. (This is also particularly interesting because it highlights people's oft-confused thinking about "nature-nurture", since physical changes can be the result of things that happen in the environment -- both physical things and cultural

things, some prenatal, some postnatal -- which fall in the "nurture" side of the debate rather than being simply the result of genetics or "nature". This is something that Lewontin has been hammering on for at least 30 years, and is particularly interesting in animals like humans, with our unusual amount of postnatal development.)

I should also mention here about Morgan's use of the terms "Darwinism", "Darwinist", and "Darwinian". There tends to be a divide about who uses these terms and this can be confusing; they're fairly commonly used among scientists in the UK but in North America they tend to be used just by creationists, who do so as a tactic, an attempt to claim evolutionary studies are a religion-like cult of Darwin. Ironically Morgan here is using a somewhat cultlike view of "Darwinism" as a good, and specifically just Darwin's Natural Selection, ignoring all those other possibilities I just mentioned. As she castigates science for "turning away" or "abandoning" "Darwinism", it sounds uncomfortably like just what creationists accuse science of doing, embracing a cult of unchanging Darwin. I doubt that's what Morgan would like to see happen, but she's entangled herself in her own arguments over the years in an unfortunate way and she's sort of painted herself into a corner.

Page 46 starts "Part Two: Objections and Replies"; should be interesting.

*"1. "The palaeontologists have found no confirmation of it."
They have found nothing that proves it and nothing that disproves it. They have found things that disprove the Savannah Theory."*

Well, no. But there's the utility of having a handy strawman theory in opposition to her own instead of the real theories paleoanthropology has worked with. The actual theories used in paleoanthropology have grown stronger and had large amounts of refinement due to new information; replacing those with her own strawman allows her to claim that not having any new information confirm her idea for decades is a big plus whereas in the real world it's clear this is a minus. There's also the uncomfortable reality that not only has much of the AAT/H "fact" base been eroded by new finds, many of those "facts" were *known to be untrue at the time they were first used by AAT/H proponents!* (Examples include but are not restricted to the diving reflex and salt hunger)

"2. "The hominids could not have lived by the water because of crocodiles."

She does her usual thing re predators, the same dismissal of the problem she's tried since she first started addressing this problem (some 2 1/2 decades after she started writing about the idea). She's seen my writing on this, so she knows better,

but still does the same ol' same ol'. At least she no longer calls crocodiles "hypothetical", which was her first attempt to dismiss the problem. (And it's not just crocodiles but also sharks as well as a variety of microorganisms.) I won't repeat the info on my pages on predators, [go there](#) for the specifics.

"3. "If our ancestors had been aquatic we would be more stream-lined."

Makes sense since it's a common aquatic and semi-aquatic trait. In answer she says we *are* streamlined, which is a mainstay of the AAT/H originating with Alister "Humans are shaped like boats" Hardy, and she does so by describing the picture of a diver she used in her 1982 book, *The Aquatic Ape*. It's nonsense. Copying from my Hardy page which describes this picture: Morgan has offered as evidence a picture of a human in bathing suit and cap diving with her arms held before her versus a picture of a sitting chimpanzee. But a more direct comparison, of similar poses, would show two animals with shoulders which jut out, rather spoiling the streamlining. The motions we use when swimming upset this streamlining even more, as opposed to the very different types of motions seen in aquatic animals. And while whales and seals are actually streamlined via their fat deposits (otters too, which carry much of their fat in their tails which aids their streamlining), the fat deposits of humans stick out in the most inconvenient way when it comes to being streamlined. Breasts, buttocks, all that fat just makes our shapes a bit too lumpy to be streamlined.

"4. "Morgan keeps changing her story. In the beginning she said one thing, now she is saying another."

Well, it's more like she says the same thing even after it's pointed out to her that she's wrong on the facts, and she claims to have dropped things but doesn't. There is the ZING!ability problem I mention in the logical Fallacies section on my "[General Problems with the AAT/H](#)" page, but that isn't so much saying one thing then and another now as it is saying whatever comes to mind to counter one objection and then pretty much immediately saying another, contrary, thing to counter another objection (this is the *ad hoc* fallacy.)

"5. "The people best qualified to judge are against it."

The people considered best qualified to judge are the people who have spent most of their lives learning and teaching about the current orthodoxy. They are naturally the ones most resistant to change. And they are in a position to ensure that supporters of change find it hard to get promoted or have papers accepted for publication."

Great Anthropology Conspiracy alert! I think the reality is that those who've actually looked at it in detail know that the facts don't line up in favor of it, and for that

matter are often reported inaccurately by the idea's proponents. And those in academia who read about it generally see some major objections in it that fall within their areas of expertise, objections so big and so egregious that they can't see the rest being worthy of a lot of their limited time, energy, and money. That's what I've seen in my experience with anthropologists and the AAT/H.

"7. "I know some people who believe in it and also believe in astrology and ESP and all that stuff."

Not really seen this objection, although I'm sure someone, somewhere, has made it. Morgan mentions A. R. Wallace's spiritualism and Newton's alchemy, but oddly enough not Alister Hardy's spiritualism and his enthusiastic embrace of some of the great frauds of psychic studies (such as Basil Soals, who Hardy thought was a fantastic example of real psychic ability). That doesn't demonstrate that Hardy's other work is equally fundamentally flawed, but it does show that he had some incredibly poor judgment on some of what he considered his important work. His general marine biology work (and his invention of the Continuous Plankton Recorder) has stood up well, while his spiritualism and his AAT/H have not.

"8. "Some of them have this Eureka moment when they hear of it and don't look for any evidence. That's no way to do science."

This certainly can't (and isn't, so far as I know) said about the principle proponents of the idea. Rather, it's said -- accurately -- that they look only for evidence for it and not evidence against it, and mangle and twist the "evidence" for it, since that "evidence" usually isn't for it until they give it their special twist. Even Morgan has said that she approaches the subject like a lawyer preparing a brief, which means highlighting anything that sounds good for your case and burying contrary information -- in doing so, she simply buries contrary evidence (as she did in her earlier claims about tears material), or bends and twists it until it she can claim it says the opposite of what it does (as she did with Denton's research on salt) -- which is definitely "no way to do science".

"9. "The picture she gives has got vaguer as time goes on, instead of clearer."

She says this is true of all paleoanthropology, which is laughable. I think the problem on the AAT/H side has always been that she, as well as the other proponents after Hardy, were *always* very vague about just what they meant, about the times involved, about how aquatic these aquatic apes were, how they dealt with predators, etc. Hardy did the best on this, but then his basic idea was that hominids were aquatic for more than twice as long as hominids have existed.

Page 49-50: *"I remember when David Pilbeam could point out that all the hominid fossil remains discovered up to that date could be contained in a shoe box."*

Mighty big shoe box, even then. Imelda Marcus shoe *closet* maybe. Actually, I haven't seen any Pilbeam remark about shoe boxes; he did give a vaguely similar remark once which Richard Leakey quoted which has been used by quote-mining creationists ever since. Various forms of the "shoe box" claim have been used over the years, always, as far as I've seen, only when talking about certain specific types of hominid or prehomonid fossils, not all hominid fossils. For instance, I've seen it used some years ago when talking about hominid fossils older than 4 million years, which were indeed very rare at that time, and I've seen it used when referring to fossils of the extremely old prehomonid *ramapithecus*.

And in fact the actual quote from Pilbeam, which doesn't mention shoe boxes, *is* about the extremely old hominid fossil period between what we had in 1981 when he said it and the last common ancestor, what has been called the "fossil gap" or here called the "fossil void" by Richard Leakey, in whose 1981 book *The Making of Mankind* the quote is reported. (At that time this gap was from about 4 millions years ago to about 8 million years ago; it has shrunk quite a bit since.) Here's the quote: "If you brought in a smart scientist from another discipline and showed him the meagre evidence we've got he'd surely say, "forget it; there isn't enough to go on." What's most disturbing here is that this quote is used by a host of creationist sources in *exactly* the dishonest way Morgan does. Creationists are generally the most dishonest people around when it comes to writing about evolution, and it's truly disturbing to see Morgan adopt their tactics so often.

Also, there's been quite a lot of fossil evidence found from that period since then (it's been over 25 years, how could there not?) so the quote isn't even accurate about that period anymore. Morgan, apparently, doesn't care.

So that's her attempt to deal with objections to her idea; a mere 6 pages and not much real content in that little. All in all, underwhelming.

Page 52 starts "PART THREE

What makes Human Bodies Special"

Bipedalism: *"we are still liable to suffer from chronic backaches and other consequential disorders unknown in other mammals."*

Simply untrue, and can only be said by people who haven't bothered to actually look at the facts before writing. Have I mentioned that this is okay in barroom conversation but not in science? And that even in the barroom it leaves you open to

losing a bet from a knowledgeable wag. Hint: if you find yourself in a bar sharing drinks with a veterinarian, don't try to win a bar bet using Morgan's claim.

"On the savannah walking erect enables you to look further into the distance. Meerkats do it all the time: it makes it easier for you to see an approaching predator."

"In the wild, chimpanzees on the edges of the forest sometimes stand on two legs to pick fruit."

"Bipedalism frees your hands for other purposes, such as making tools."

"It was a protection against overheating, because if you are upright, a smaller percentage of your body surface is exposed to the perpendicular rays of the mid-day sun."

"They needed to carry things."

"A man walking upright uses less energy than a chimpanzee walking on all fours."

"It was for reaching up to gather the seeds from tall grasses."

"Rearing up on two legs was a signal to other males, to intimidate them. We can see gorillas doing it."

"We may not be able to outrun a deer, but if we follow it for twenty or thirty miles, we have more stamina and we can tire it out."

Now note that all these things are true, and are certainly good reasons why bipedalism has been effective for our species and therefore have withstood the test of selection. Note too that she twists these to try to make them sound foolish or otherwise make them sound less compelling. For instance, when she says *"Rearing up on two legs was a signal to other males, to intimidate them. We can see gorillas doing it."* this is undoubtedly true but incomplete because in fact virtually *all* apes and at least many monkeys (as well as many other animals) do this, use bipedalism in displays. That hominids wouldn't do it, and have it contribute to selection pressure for bipedalism, is a bizarre thought. And when she says *"In the wild, chimpanzees on the edges of the forest sometimes stand on two legs to pick fruit."* this is also true, but again incomplete and misleading because chimpanzees in all areas sometimes use bipedalism when gathering -- and carrying -- food (not just fruits). In fact most studies show it's overwhelmingly the most common time for chimpanzees to use bipedalism. When she says *"A man walking upright uses less energy than a chimpanzee walking on all fours."* she leaves out the vital information (which she has been informed of) that *even for chimpanzees*, which are adapted to

walking quadrupedally, walking bipedally uses no more energy than chimpanzees walking quadrupedally.

If hominids were, due to more effective bipedalism, able to more effectively display, and observe, and persistence hunt, and gather foods, and carry foods, and carry tools, and at the same time get better protection from heat, doesn't that sound like a suite of reasons that makes effective bipedalism a good thing for hominids and therefore not subject to elimination through natural selection? (Tanner also suggested that sexual selection, via "inadvertent" displays by males and consequent selection by females, played a part as well.) Wouldn't a long list of good reasons for something be better than one item alone? AAT/H proponents, for some reason, have long resisted this obvious conclusion.

Page 57: *"There is just one situation in which all apes and monkeys resort to bipedalism. They all do it whenever they have to cross a stream, or ford a river, or wade into a swamp to gather some of the succulent plants that grow in the water."*

This has become *the* classic Algis Kuliukas falsehood (Kuliukas is, along with Verhaegen, the most prominent AAT/H booster of recent years, especially online), and we see that she got it from him because she immediately follows up by using Kuliukas's thesis claim of percentages, and just like Kuliukas she doesn't mention that his findings -- from his 5 hours of observation of captive bonobos (and 37 seconds of bonobos wading) --- are way out of line with every other study, most of which involved longer periods of study, often of wild bonobos. When you have a study which is off the graph compared to every other study, you really have to confront this problem and look for explanations -- that's science. Not doing it -- not science. Anyone still wondering why the AAT/H hasn't taken academia by storm? (More details on Algis Kuliukas and his thesis, etc. on my [page on him](#).)

To get back to the claim itself, it's simply untrue. Sometimes apes and monkeys will walk bipedally when crossing a stream but most of the time they don't, plus they don't cross streams all that often, far less often than they use bipedality in those other situations that Morgan derides. In fact, it seems that most of the time that apes, at least, use bipedality when they are in water they are feeding, which is a major time they use bipedality on land; so which reason is it they're using bipedality in that situation, food-getting or the mere fact of being in water? The fact that they're usually quadrupedal in water, and that they often use bipedality when food-getting on land, suggests that food-getting is the more critical reason in both cases. But certainly at some times apes and monkeys are bipedal when in water, and in some very few occasions it's not for other reasons, so it's only reasonable that it be placed on the long list of occasions when primates sometimes use bipedality -- that long list which Morgan derides. Being on this list is not what Morgan and other AAT/H proponents want -- in fact they rail against it -- they want

it to be *the* reason for bipedality, which isn't reasonable because it just does not fit the facts.

Pages 60-67 are on the larynx. She briefly mentions, without naming names here, Fitch's work with red deer, but not the many other species Fitch mentions. Also mentions the chimp descended larynx paper by Nishimura, Mikami, Suzuki, and Matsuzawa, but dismisses it. She also dismisses Fitch's conclusions, even though in doing so she has to dismiss his extensive, well researched and well thought out -- eminently sensible and scientific -- work. She seems to think the answer to larynx descent can only be mouth-breathing (which I think she gets from Verhaegen; he does go on about mouth-breathing). Bottom line is that she wants to somehow keep her earlier claims about the larynx in spite of the fact that they made little sense when she first made them and now -- with Fitch's and others' findings about the existence and most likely purpose of the descended larynx in a variety of unrelated species -- make no sense at all.

Page 66: *"Incidentally I believe, but have been unable to verify, that we are the only terrestrial species equipped with the gasp-reflex that makes us respond to sudden startlement with a swift intake of breath. It is as if our autonomic reaction to sudden danger was: "Get a lungful of air at once. It might be some time before you can get another." No land animal has evolved to cope with that possibility."*

One thing to note about this reflex is that it's extremely helpful if you really, *really* want to drown (it's mentioned in sports medicine as a major cause of kayakers' drownings). Along with our reflex, suppressed in actual aquatic animals, of breathing in when we run low on oxygen even if we're underwater. Odd adaptations for a supposedly aquatic mammal whose major features supposedly came from an extended period (millions of years) of aquaticness.

Also, regarding "belief" versus "verification", I did less than 5 minutes of online searching and found references in scientific papers to the gasp reflex being present in both lambs and macaques. I would think that it, like the startle reflex and many other reflexes, is a very old ancestral reflex and likely present in a great many, if not all, mammals. Unlike Morgan, however, I choose not to substitute my belief for actual data, especially when you can find that data from any Internet connection in a matter of minutes.

Page 67-68: *"Only a few mammals are able to exercise this kind of control over their breathing. They are all aquatic. A seal before it dives decides how deep it intends to go and how long it is likely to be before it is able to take its next breath, and acts accordingly."*

First this seal reference she makes is not about breathing control, but bradycardia (the slowing of the heart rate); I describe this Morgan mistake (which she's been making for nearly two decades now) on my page "[Can AAT/H proponents research be trusted?](#)", in the section "Morgan quotes Elsner and Gooden: Accurately?" As for conscious control of breathing, it is apparently an accidental by-product of (and another advantage of) bipedalism; in quadrupedal animals the muscles which control breathing are bound up with locomotion; when you don't need these muscles for locomotion they are freer to be used consciously. She mentions Langdon explaining this (on page 69) but dismisses it or doesn't understand it (*"Without some compelling reason, being en-dowed with conscious breath control would be no more liberating than being given conscious control over the bile duct."*) And Morgan denies this is true in apes (*"True, respiration is locked in phase with gait in some quadrupeds, but that does not apply to the great apes."*) but gives no reason for thinking so, nor any reason for anyone to believe her claim. Lots of people are unfamiliar with, or uncomfortable with, the role of chance in evolution, and this is an interesting and important example of how that chance can affect important later adaptations. That bipedalism allows conscious control of breathing and therefore provides an important component of speech is important, but is almost certainly just a lucky break, an extra benefit that wasn't immediately apparent.

Pages 70-73 are on fat. She once again uses the statement: *"Homo sapiens is endowed with ten times as many adipocytes - the cells that store fat - as would be expected in an animal of our size"* without (again) seeming to understand what Caroline Pond was saying when she said this, even though Pond was very clear and explicit about what she meant and others (ie. me) have explained it to Morgan again and again online. It's also on this site on my page on "[Fat](#)". From there, briefly, the implication (sometimes said explicitly) by AAT/H proponents is that we are 10 times fatter or necessarily have 10 times more fat cells than our primate relatives or some unspecified "savanna" animal. This is not what Pond was saying at all. Pond's article and her statement about relative number of adipocytes was aimed at pointing out that the usual models for human fat studies -- rats and mice -- are inappropriate because they have relatively large and few adipocytes which then expand and reduce as total fat goes up and down. Humans, as with other primates, have relatively small and numerous adipocytes and when we and other primates get fatter we tend to add more adipocytes since they can't expand as much as those of rats and mice. This makes it harder to lose total fat compared to those rodents.

Page 71, Morgan referring to Caroline Pond again: *"But she refuses to refer to the subcutaneous site as a "depot". It is as if its presence there was a kind of aberration, as if fat from more primal and genuine sites has merely happened to*

migrate, as it were illicitly, to the surface of the body and spread out there to form a continuous layer."

Caroline Pond refuses to refer to it that way because doing so would be inaccurate, and in fact the fat we see under our skin, just like the same layer of fat we see under the skins of other primates which are allowed to get fat, has indeed spread out from the internally anchored fat depots they are part of to "the surface of the body".

"Her opinion is that the fat "may simply be a consequence of our large body mass, as it is in large, obese carnivores such as bears."

Pond does say this about superficial fat, and she demonstrates this physiological tendency in a graph which is based on extensive research on a wide variety of animals.

Morgan seems to be trying to "disappear" Pond's main conclusion, stated more than once and ignored by Morgan as often, that human fat characteristics indicate that it is a) evolved for shaping and due to sexual selection, and b) more abundant due to the fact that humans have, for a great many years -- since the use of fire and "advanced" weapons such as spears -- suffered relatively little predation and that, just like other animals which suffer relatively little predation, tend to have more fat than their near relations in the animal world. Fat is a tremendous resource in lean times, but it slows one down, which makes the animal more subject to predation. So animals tend to put on as much fat as possible, with predation tending to control the upper limit; remove or drastically reduce the predation and the population gets fatter.

About Pond's book (which *is* a fascinating book, by the way) Morgan says: "In short, this is a fascinating book, but I get the feeling that it is trying too hard not to rock the boat."

First Morgan says Pond is bucking the general idea among scientists that fat is primarily adapted for insulation (and Pond has good reasons for doing so, which Pond explains, and which are outlined on my "Fat" page) and that Pond is again bucking a general idea among scientists when she says that fat is not attached to the skin as a depot (and Pond has good reasons, which Pond explains, for thinking so) but nevertheless is "trying too hard not to rock the boat". Wonder how much Pond would disagree with standard scientific thought about fat if she wasn't "trying too hard not to rock the boat."

Brief mention of Cunnane and Crawford on page 73. I go over the problems with their conclusions on my "[Omega-3 gang](#)" page.

Page 74: *"We are furless".*

Wow!

We're like apes!

That's so weird!

Seriously, GIGO.

Furlessness is a general ape/human trait, in fact less body hair than non-ape/human primates is a general ape/human trait. You have to describe what you're attempting to explain accurately if you want to have any hope whatever of explaining it.

She poo-poos -- once again -- Darwin's idea that much of the reason for human hair characteristics is sexual selection. The fact is that although Wheeler (and Jablonski who turns up in Morgan's book shortly) have shown how human body hair is useful for sweatcooling, many human hair characteristics -- variation within the species and different populations, differences between the sexes, and big changes right at puberty -- just scream sexual selection. It's a classic case, and this isn't good for the AAT/H, which depends heavily on this fact not being so. So Morgan discards it out of hand, as she's been doing for many years.

Page 75: *"How is it possible that a patch of the same hair that is overheating the body could be so effective at cooling the head?"*

As an experiment try these three situations on a hot day:

1. no shade whatever
2. shade the top of your head with a piece of cloth or cardboard
3. use the same type of cloth or cardboard (say a close-fitting coat, or a tent or cardboard box) and shut yourself up inside it

Which is cooler? Morgan's question implies that situations 2 and 3 will feel the same; they won't.

In pages 76-81 Morgan attempts to come to terms with Nina Jablonski's work on skin.

"Jablonski's critique is clearly and fairly presented. I have only two small quibbles. In the notes she refers to her source of information about the Aquatic Theory as 'Elaine Morgan 1982.' That early and amateurish effort of mine has long been superseded by one published fifteen years later."

Once again Morgan says that one of her books (written after a decade of her working on the idea) was "amateurish". How she can get mad about others pointing this out when she does it herself is odd, but there you go.

Morgan finds it more acceptable that while Jablonski rejects the AAT/H, she doesn't do so merely because it is "not, however, supported by facts". Morgan does not say why she thinks rejecting something that isn't supported by facts is such a bad idea.

Morgan also objects to Jablonski saying that body shaping rather than insulation is behind whale fat, but this has been shown by Pond and others through observation and experimentation (and this also has been pointed out to Morgan in the past).

On page 78 Morgan claims -- absolutely falsely -- that Jablonski *"takes the view that once sweating had been selected for, hairlessness had to follow, because sweating would only be effective in the absence of body hair"*. Morgan has a long-standing habit of misrepresenting what researchers say like this, always rewording what they said to make it easier to shoot down (the Strawman logical fallacy). What Jablonski actually does say is that not having a thick coat of hair aids a great deal in cooling via sweating, that animals with coats of hair can and do cool via sweating but that it just isn't anywhere near as efficient as it is in humans due to our body hair characteristics. This, described by Jablonski, is simple physics:

"The evaporation of sweat cools an animal because heat is lost from an object when liquid vaporizes from its surface. The most efficient evaporative cooling afforded by sweating occurs right at the surface of the skin itself. But if an animal builds up a sweat and its coat gets wet, most of the evaporation will occur at the surface of the coat and not at the surface of the skin. this leads to a buildup of heat in the body because heat from the blood vessels in the skin must be transferred to the surface of the wet fur rather being dissipated quickly from the skin's surface. As a result, the animals ends up sweating much more, and its fur gets wetter in order to achieve an adequate degree of cooling. Physiologically, this is highly inefficient and nearly impossible to sustain for any length of time. Unless the animal can drink water regularly while it exerts itself, it collapses of heat exertion."

(This is part of what allows humans to use persistence hunting to run down and catch otherwise much faster prey.)

Jablonski again:

"A hominid with a thick coat of hair would have a hard time keeping cool when it was highly active because its wet hair would act as a blanket, impeding the loss of heat from the skin's surface. The body's efforts to produce more sweat in a vain attempt to keep cool would then result in rapid fluid loss. Most authorities now agree that these are precisely the conditions that triggered the evolution of hair loss in the human lineage.

Remove most of the body hair and the problem of evaporating sweat from the surface of the skin disappears."

Morgan may be thinking here, as many people mistakenly do, that evolution searches for optimal solutions to any given problem. It doesn't. Evolution tends to provide "good enough" solutions, some of them quite elegant, but not optimal.

In terms of Morgan's tactics, there's an interesting passage on page 78: *"Most animals have a long history of being at the mercy of parasites which have been extant almost as long as their hosts, without leaving any detectable genetic evidence of such assaults. The intriguing fact is that three of the ectoparasites which have specialised in living on Homo are unable to complete their their life cycle unless and until their host enters the water."*

The first sentence here is simply Morgan making a sweeping statement without the slightest backup; simply accept her claim -- why? because she says so. The second is more subtle and very much like others she's used when she's been confronted with evidence that doesn't actually support her position (for instance on hymens, or on tears and sweat in her 1997 book); just insinuate there's some evidence there, refuse to explicate it, and try to leave a vague impression that this constitutes evidence for her position without saying enough to be challenged. As I mentioned before regarding her claims about hymens, this also allows her to act offended if someone points out she's still using these statements as if they were, in some unspecified way, evidence for her position.

Specifically on the ectoparasites she mentions... well, since she doesn't mention any it kind of makes it hard to be specific in critiquing her claim, doesn't it? The statement seems to imply that these are parasites which live fulltime on humans and that humans have to be in water for some time for these parasites to complete their life cycle, and that these live only on humans. Without specifics what can one say? Well, let's look at one such ectoparasite, the one causing schistosomiasis which Jablonski mentions and is therefore likely to be one that Morgan was referring to (note that Morgan's not mentioning that this is what she's referring to will allow her to act offended at my presuming this is it; that's part of why I refer to this as a tactic). The parasite is *Schistosoma mansoni*, a fluke which lives for part of its life in freshwater. It starts out living in a freshwater snail, then freeswimming, then enters a host when it encounters skin. Morgan's statement would imply that this skin must be human, but actually they infect various non-human animals in addition to humans. It doesn't require lots of water contact; in fact in Egypt they have problems with irrigation because of *Schistosoma mansoni*. Of course using irrigation or washing clothes in a water source now isn't being semi-aquatic, and neither is going to water to drink or the occasional wading or swimming at any time in our past, but that's all it takes to fall victim to *Schistosoma mansoni*. Morgan's

statement also suggests or implies something that's a false dichotomy: either "aquatic ape" or no contact whatever with water. If this statement of Morgan's isn't merely a tactic instead of an honest if incredibly vague attempt at an argument for her position, the oddest part is Morgan's thought that susceptibility to a debilitating disease or condition due to contact with water is evidence that we are adapted to heavy contact with water for millions of years. She's done something similar to this in the past with the apparent vitamin A poisoning in KNM ER-1808, and Marc Verhaegen has done it with ear exostoses (described on my page on him). That doesn't make sense.

On page 79 Morgan, as is usual for her, imagines that water is only available at waterholes or streams, when the reality is that this is not the only place one gets water in dry places. This is true both of humans using stone age level and pre-stone age level "technology" as well as chimpanzees. For instance there's those lianas I mentioned above, as well as water-filled tubers which are dug up and carried both by humans and savanna-dwelling chimpanzees; sipping from water deposits in nooks and crannies or trees, bushes, and rocks, and sponging up water with crushed leaves -- done by both humans and chimpanzees. Morgan's false claim is just an attempt to mislead by use of a false dichotomy (either dangerous waterholes and streams or no water). Of course this also conflicts with the common AAT/H idea that the waterside is safe, but contradicting itself has been a mainstay of the AAT/H for decades. She also compares humans with camels -- she has a thing about camels, apparently, always wondering why we, an environmental generalist not specifically adapted to any one environment and capable of living reasonably well in almost all environments, aren't like desert-adapted camels (in this case when it comes to how fast we can drink water).

On page 80 Morgan says *"When the word 'savannah' is used it is carefully preceded by 'woodland' as if in recognition of the revelations of the nineties."*

Or is it as if in recognition of the generally accepted meaning of "savanna" in science *for well over a century* (since 1872), as mentioned on my page on ["Definition of savannah"](#).

Page 80 again: *"There is a passing reference to the least convincing of the savannah's Just So stories - the legend that this primate decided it would be a good idea to stay active under the mid-day sun while all the animals with any sense retreated to the shade."*

Morgan here (like a lot of people do) gets confused by the fact that, in a case like this, an adaptation can allow an animal to use a specialized niche, and that this may later also be useful in new areas and ecological niches, which increases selection pressure for that adaptation, which allows better use of that niche and

others, and so on in a feedback loop. In this case the ability to use hot areas during their hottest times better than other animals opens up a niche, and allows other, later, behaviors such as persistence hunting. It doesn't have to be (and no doubt almost never is) anything like a deliberate decision, but instead a lucky break that a chance change, or a change which at first was due to some unrelated reason, becomes useful for additional reasons, creating a feedback loop for more effective physical change which allows for more effective behavioral change which allows... well, you see how a feedback loop works.

Pages 80-81: Morgan suggests we should've just abandoned our primate heritage and "installed" an antelope brain-cooling system (from the Big Catalog O' Adaptations, no doubt... which is now available online, I hear. I'm ordering up some gills as soon as I find the URL, which I am having some trouble doing; can anyone point me to this site where you can ignore phylogeny and install heritable bits from any animal?) This is of her longest running and most ridiculous errors (you'll remember she started this book by wondering why we didn't forsake our primate phylogeny and become like camels; it's an old question of hers which she repeatedly repeats in a repetitious manner). When someone writes ideas like this for decades, while claiming to be doing science and insisting science throw out scientific ideas which reflect reality and accept these foolishly flawed ideas instead, can anyone be surprised that scientists don't jump at the chance?

In the last pages Morgan offers up all the various less aquatic aquatic ape ideas as somehow better but doesn't note -- which is hardly unique to her among the ideas' proponents -- that this makes it harder to explain the idea rather than easier. The essential problem is that people have been using as major examples of aquatic traits "present" in humans characteristics found only in whales, seals, and sirenians -- hair, fat, sebaceous glands, etc. They describe these characteristics inaccurately, so they aren't the same at all, and therefore run headlong into GIGO, but the other problem is that they're now trying to say that through convergent evolution we grew to resemble animals which have been fully aquatic for tens of millions of years (longer than hominids have existed) by even less semi-aquatic behavior than before. How do you evolve to be like a seal just by walking along the seashore and taking the occasional dip? You don't.

She gets kind of mad when she talks about the reaction to her comparisons of her idea's acceptance level with Wegener's continental drift, but she misses the point, whether through not getting it or as a tactic I don't know. But I cover that problem on my [page about Wegener](#). She also claims he was seen as a crackpot, but as I describe on my Wegener page that is simply not so.

This is the central conceit of her book, from which she gets her book's title.

It's a short Freudian slip of a section where Morgan uses the tale of the Emperor's new clothes as if she plays the part of the brave little outspoken boy and not the emperor whose marvelous new set of clothes is spun out of nothing. (This might have made some sense if paleoanthropology had not shown *it* makes sense by making made steady progress in its lines of research.) I'm afraid what Morgan is doing here is engaging in projection, she being the one who spun a supposedly marvelous set of clothes out of special data that to informed and unbiased eyes is transparently false. In fact, her book is, in effect, a long version of what PZ Myers has called "The Courtier's Reply". PZ's "[The Courtier's Reply](#)" is a short essay written as a parody of one of the standard responses to Richard Dawkins' views on religion, in which one of the emperor's courtiers defends the ruler's garments from the brash accusations of those who dare to believe their own eyes. The courtier haughtily announces that no reasonable person could say the emperor's clothing was not there unless they had studied the extensive writings about "proper appreciation of Imaginary Fabrics" and the like, just as the typical apologetics response to Dawkins' simply asking for proof of a god before acceptance of that god is to insist that this stance is untenable unless he has first studied various apologetics explaining why that proof is not forthcoming. Since Morgan has built her idea out of false "facts", the analogy to "proper appreciation of Imaginary Fabrics" is especially apropos.

Morgan simply announces that her research, while self-admitted to be often "amateurish", "sketchy and superficial", is to be accepted in place of actual work done by thousands of actual researchers, and that when push comes to shove, and you have to choose between her superior "appreciation of Imaginary Fabrics" or your own lying eyes, you're supposed to pick her.

So, book read, it turns out not to have anything new, to be simply a regurgitation of her previous works in shortened form, with even less substance and more whining (whining for you Brits), the insistence that the level of argument minimally acceptable in a barroom conversation should have swayed science despite the many errors of fact and misunderstandings of science it contained, and the complaint that academia viciously ignored her and her ideas via the truly dastardly method of repeatedly hosting her giving talks on that subject (the bastards!).

Then Morgan ends by suggesting a favorable comparison of herself and her idea to Mendel, Dart, and Wegener is in order, apparently forgetting that to obtain true netkookdom one simply must invoke either Galileo or Einstein, preferably both, as her fellow AAT/H proponent Marc Verhaegen so often does. Her concluding Richard

Feynman quote ("We have a way of checking whether an idea is correct or not that has nothing to do with where it came from. We simply test it against observation.") is, of course, is unintentionally ironic since it is by comparing her claims to observation that people have decided those claims don't hold water.

I feel I should add here at the end that this critique is an example of a problem I point out on another page of my site, "[Why don't professionals spend their time refuting the AAT/H?](#)", that critiquing the AAT/H is not worthwhile for professionals because it has so many errors and misunderstandings of theory. You can see it reflected in the length of this critique, which is somewhat over half the length of Morgan's book! I know I can be longwinded, but no book purporting to be science should contain so much wrong that it takes anywhere near that much verbiage to correct it. Is it any wonder that the AAT/H hasn't been embraced by scientists as Morgan obviously so devoutly wished?

Review/Critique of Elaine Morgan's 2009 TED Talk

On 22 July 2009, Elaine Morgan gave a talk at the annual TEDGlobal conference in Oxford, UK, in a session titled "Curious and curiouser". It's garnered a lot of notice (and drove a lot of traffic to my site) so I figured I should write up a response specifically to that talk.

The first thing that struck me happened even before they put the video of her talk online. I looked at the TED site and read the blurb on Morgan, and they highlighted a quote from a writer in the *Sunday Telegraph*. This is also prominently displayed on Morgan's web site, indicating that she included it in her PR packet for the TED talk. I hadn't paid much attention to it before, but what struck me was how anyone could use this for self-promotion, how on earth they could consider it a compliment. Here it is:

"For my money, she is more scientific than Genesis, more up-to-date than Darwin, more fun than Ardrey, and she writes better than Desmond Morris."

John Rowan Wilson, *Sunday Telegraph*

Now take that one step at a time:

1. "more scientific than Genesis". Unless you're talking about the Genesis probe from *Star Trek III: The Search for Spock*, what isn't "more scientific than Genesis"? That's an extremely backhanded compliment at best; if someone

said it about me I'd regard it as a not very subtle insult.

2. "more up to date than Darwin". More up to date than a guy who died 127 years ago. Again, who isn't? Insult number two.
3. "more fun than Ardrey". Possible compliment, although I never liked Ardrey's stuff so I'd consider a low bar to clear. Interesting parallel though, since Ardrey was a screenwriter who thought up and wrote about poorly supported pop ideas of human evolution that ultimately didn't agree with the facts. At least Ardrey had the excuse that there was less good info when he was doing his stuff.
4. "writes better than Desmond Morris". Although I'm also not a Morris fan, to say the least, many people are and I think this is a decent compliment. One which you'll note mentions only Morris' writing, not his knowledge or research.

So this backhanded compliment/insult is apparently, judging by her prominent use of it, thought by Morgan to be a marvelous thing to have said about her.

After a few more days the TED people put the video of her talk online and I watched it, so let's get to it. One thing; the official transcript they have on their site doesn't match her talk in several spots. My transcription was done by me while listening to the talk and going over any difficult sections several times if necessary. It looks like their transcription was done after the fact as well but not as carefully checked. No really huge problems with it, but a couple of small things, like their saying she used the word "dicta" (several times) near the end when she clearly said "diktat", which gives a very different meaning to an important point.

Morgan's words are marked by what time they appear in the video and are in italics, mine aren't. I've indicated some of her accompanying gestures in brackets.

[0:40] *"Cause there's one aspect of this story which they have thrown no light on and they seem anxious to skirt around and step over it and talk about something else. So I'm going to talk about it. It's the question of why are we so different from the chimpanzees?"*.

Yes indeed, paleoanthropology and primatology have done virtually nothing whatever in the last half century. All those thousands of researchers, all those expeditions, all those hours in zoos and the wild, at fossil sites, in the lab, and poring over books and journals; all that never happened. What can you really say

about someone who says something like that? Does she actually believe it, or is she just trying to snow a gullible audience*? Either way, it's not a good start.

* (TED audience members pay \$6,000 to attend, although TED is quick to claim this isn't much because they allow 20 people to pay only \$2,000, and starting this year allowed 30 freebies among their audience of over 1,400. In addition, they have to apply to be members of the audience, which includes answering questions they say takes about an hour (!). It's a carefully vetted audience. That doesn't necessarily make them gullible -- although it might make them a teensy bit more likely to appreciate what they're given -- what makes them gullible is that, as their reaction shows, they accepted what Morgan said without reservation.)

[1:32] *"That one is a naked biped. Why?"*

Could someone in the AAT/H camp do some minimal research to get this right... I don't know, watch *The Fisher King* or something where Robin Williams takes off his shirt? Have none of these people ever been to a beach? Do none of them notice that shaving with razors is a \$2 billion dollar a year business, and that electric shavers generate billions more each year? Why does *The Boston Globe* report that "In Reading and at other sites, P&G is devoting more resources to emerging hair removal technologies for women, which is an estimated \$10 billion industry worldwide." Why do people without hair buy these things?

If you start with a wrong description for your question you *will* get a wrong answer. Guaranteed. GIGO.

Morgan has always insisted on using a wrong description and therefore asking a false question, which means she has always guaranteed she cannot possibly get the right answer.

[1:48] *"Now 50 years ago that was a laughably simple question. Everybody knew the answer, they knew what happened. The ancestor of the apes stayed in the trees, our ancestors went out onto the plains. That explained everything. We had to get up on our two legs to peer over the tall grass or to chase after animals or to free our hands for weapons and we got so overheated in the chase we had to take off that fur coat and throw it away. Everybody knew that for generations, but then, in the 90s something began to unravel. The paleontologists themselves looked a bit more closely at the accompanying microfauna that lived in the same time and place as the hominids and they weren't savanna species. [does "puzzled" frown] And they looked at the herbivores and they weren't savanna herbivores. And then they were so clever they found a way to analyse fossilized pollen. Shock horror. The fossilized pollen was not of savanna vegetation. Some of it even came from lianas, those things that dangle in the middle of the jungle. So we're left with a situation where*

we know that our earliest ancestors were running around on four legs in the trees before the savanna ecosystem even came into existence. This is not something I've made up, this is not a minority theory, everybody agrees with it.

Perhaps by "then in the 90s", she means "1890s", because it was before that when pollen was studied in the fossil record. And certainly both microfaunal assemblages and pollen were extensively studied and used to reconstruct ancient environments decades before Morgan claims. I mean really, what does she think Noel Boaz did with the grant money he got from the L.S.B. Leakey Foundation in 1979 to study "Microfaunal Sampling at the Upper Neogene Site of Sahabi"? Since he couldn't, in Morgan's world, actually been doing that study, maybe he went to Vegas? Maybe he bought a new Porsche? Someone from the Leakey Foundation should really look into that one. What was J.C. Ondrias doing *really* doing when he claimed to be studying microfauna in Greece in the mid-1960s? When they started excavating at Sterkfontein in 1966, collecting microfauna in their work, what were they doing with it -- since according to Morgan they could not have been studying it. How could Estella Leopold write about paleo pollen analysis in China in 1977 in the book, *Paleoanthropology in the People's Republic of China*? What was Raymond Bonnefille doing writing "Implications for pollen assemblages from the Koobi Fora Formation" in 1976 if Morgan is right that such work didn't happen until 25 years later?

BTW, I pointed out in my review of her 2008 book that, contrary to what Morgan says here, lianas grow in other than jungle (in fact they're more abundant in drier conditions) and some are savanna plants. And that took me less than 5 or 10 minutes to find out, online. So with Morgan we're talking about someone who makes claims that she won't even spend 5 or 10 minutes researching from her office chair.

At 3:46 she starts in with the paradigm thing. It's a paradigm shift folks, it's all over. Run for your lives. quickly, quick, quic.... zzzzzzzzzzz

Whaa?! fell asleep there. Onward.

Seriously, when you hear the words "paradigm shift" (or even the word "paradigm" applied to an idea the speaker or writer disagrees with) it's a huge red flag and you should brace yourself to be hit with a load of BS. Because ninety-nine times out of hundred it's coming your way in bucketfuls. And that's what happens here. Look. I've seen disagreements within paleoanthropology. Major disagreements. They happen all the time, as they do in any science that isn't long settled. That's how science gets better and better answers over time. The picture Morgan paints of paleoanthropology, the supposed stasis and top-down authority, is just nonsense.

[5:25] *And if you're going to say, "I'm going to stop talking about selective pressures," you can take "The Origin of Species" and throw it out of the window. For it's about nothing else but selective pressures.*

Couple points. One is that it's not all selective pressures. Morgan has long wanted selection -- and not sexual selection, just natural selection, and really only the narrow range of natural selection due to environment -- to be the be all and end all of human evolution. But it's not. Genetic drift is important, for instance, just as it is for any other organism's evolution. And in the paper Morgan obliquely refers to here Evelyn J. Bowers points out that "a trait can not evolve unless a useful mutation appears" and therefore asks us to "consider what kind of mutation might be involved in forming a bipedal pelvis" rather than just mull endlessly on selective pressure. Because Bowers then points out that the selective pressure needn't be so dramatic or drawnout a process as we once thought, because the change to effective bipedalism just isn't so difficult or drawnout a change as we once thought; it's not unlike changes to hair, where large physical changes come from minor genetic changes. In the case of bipedalism, all that's needed for a big physical change is fairly minor changes in a few genes which regulate development. This changes how we think about the type, degree, and duration of selection pressure needed, a critical thing to know when you examine selection. Morgan seems to find this quite uninteresting except as a field from which to extract out of context quotes.

[7:27] *"But by now everybody agrees that the elephant had an aquatic ancestor. They've come round to agree that all those naked pachyderms have aquatic ancestors. The last stopper was the rhinoceros; last year in Florida they found extinct ancestors of the rhinoceros and said seems to have spent most of its time in the water.*

The ancient members of the rhino family she's talking about -- from the Florida reference it's almost certainly *Teleoceras* -- have actually been known (their fossils are pretty common and widespread) for decades. True, they found some last year in Florida but that's like saying they found some sunburned tourists last year in Florida and pretending it's a new thing. And while *Teleoceras* was semiaquatic it was *not* ancestral to modern forms of rhinos, it was a side branch. So like the common claim that the semiaquatic *Moeritherium* is ancestral to modern elephants, this claim is false. The rest of the "elephants were aquatic" idea is also on shakier ground than Morgan claims (echoed, I must say, by far too many sources which should know better). It comes from a study of elephant fetuses which have one feature that the researchers say shows aquatic ancestry, but it's a pretty thin thread to hang an elephant's ancestry from. It may turn out be so, but it's far from a settled question

at this point; just an intriguing possibility. Morgan's rhino claim isn't even an intriguing possibility; it's *completely* bogus.

[7:50] *So this is a close connection between nakedness and water. As an absolute connection; it only works one way. You can't say all aquatic animals are naked. Because look at the sea otter. But you can say that every animal that has become naked has been conditioned by water, in its own lifetime, or the lifetime of its ancestors. I think this is significant. The only exception is the naked Somalian mole-rat. Which never puts its nose above the surface of the ground.*

In fact *most* semiaquatic mammals are hairy, and when you look at how many times aquaticness and semiaquaticness evolved among mammals, the *overwhelming* number of times it led to hairy beasts. More than 90% of the time in fact. And of course you again have to remember that we aren't hairless. You want the number of times the evolution of aquaticness and semiaquaticness led to a mammal with copious amounts of hair on it's head, patches of hair at the crotch and armpits, and hair on the body amounting to lots of very short fine hair on most of the body in some individuals ranging up to lots of thick curly hair on most of the body? Never. Not once. Not even one single time.

Let's mention the mole rat here. Morgan usually adds in the babyrusa, the wild pig found in areas of Indonesia, and claims it's semiaquatic. It's not; it swims well and can swim some distance, but that is not the same as being semiaquatic. And even if it was it using it as an example runs into the same problem as using the naked mole rat. Morgan claims these are hairless due to the habitat they live in, but if that were so we would expect other animals in that same habitat to be the same, especially closely related mammals with similar habits. Well, there are several subspecies of babyrusa and they're not all hairless as is the one we're most familiar with, yet they have similar habits and habitats. There are also five species of mole rats with similar habitats, yet only one is hairless. What these mammals provide is an example which shows that it's not surprising that one member of a group might be much more hairless than other members of that group, for idiosyncratic reasons rather than habitat. We see it with babyrusas, we see it with mole rats, and what do you know, we see it with humans.

[8:32] *"But you can say this: all the apes and all the monkeys are capable of walking on two legs if they want to for a short time. There's only one circumstance in which they always, all of them, walk on two legs and that is when they're wading through water."*

Simple nonsense, and I'm afraid I have to point out nonsense that she knows is not true.

[8:56] *"Look at the fat layer. We have got under our skin a layer of fat all over nothing in the least like that in any other primate. Why should it be there? Well they do know that if you look at other aquatic mammals that fat that in normal land animals is deposited inside the body wall, around the kidneys and the intestines and so on has started to migrate to the outside and spread out in a layer inside the skin. In the whale it's complete; no fat inside at all. All in blubber outside. We cannot avoid the suspicion that in our case it's started to happen; we have got skin lined with this layer. It's the only possible explanation of why humans, if they're very unlucky, can become grossly obese, in a way that would be totally impossible for any other primate. Physically impossible. Something very odd about our fat layer; never explained."*

We'll put the whale part of this down to exaggeration and let it slide; the rest isn't so easy to give her a pass on since she knows the facts from evolution of fat expert Caroline Pond as well as other people, including me, pointing them out -- so much for her claim that it's "never explained". Non-human primates which get fat do so in the same way humans do; their fat spreads out just as ours does. Yet Morgan calls this fact, which she has been informed of many times, as totally impossible, physically impossible.

[9:57] *The question of why we can speak. We can speak. And the gorilla can't speak. Why? Nothing to do with his teeth or his tongue or his lungs or anything like that. Purely to do with its conscious control of its breath. You can't even train a gorilla to say "Ah" on request. The only creatures that have got conscious control of their breath are the diving animals and the diving birds. It was an absolute precondition for our being able to speak.*

Two things: one is that the idea of conscious control of breathing not being found in non-human and non-diving animals is overdone at least; if it weren't then they couldn't swim underwater, as dogs and monkeys do for instance. Untrained dogs can even hold their breath, on average, longer than untrained humans. Humans do have more conscious control of our breathing than most other mammals but this is a lucky side benefit of bipedalism, which frees up the muscles around what would otherwise be our forelimbs. These muscles are also the muscles around our lungs, and since they aren't used heavily and automatically for locomotion they are free to help us in breathing control. Very lucky break, as speaking language as well as we learned to would be much harder, if not impossible, without this lucky side benefit.

Okay, make that three things, think about this: you can train a dog to bark on command, right? In fact you can teach a lot of animals to vocalise on command. And as I pointed out, many of them can swim underwater which requires them to breath in and hold their breath. So a lot of non-diving animals *in fact* do what Morgan says they *cannot* do.

[10:32] *And then again, there is the fact that we are streamlined. Trying to imagine a diver diving into water, hardly makes a splash. Try to imagine a gorilla performing the same maneuver. and you can see that, compared with gorilla, we are halfway to being shaped like a fish. I am trying to suggest that, for 40-odd years, this aquatic idea has been miscategorized as lunatic fringe, and it is not lunatic fringe.*

I'm afraid this is an unfortunate juxtapositioning of Morgan's, because the idea that we are streamlined, which she and Hardy pushed from the beginning, is one of the bigger reasons to think of the idea as lunacy. It's certainly true that trained humans can dive much more neatly than I'd expect a gorilla to do. Especially since gorillas don't dive. What an odd coincidence that she picked such an unlikely comparison rather than defend her idea that humans are streamlined. Look at yourself in a mirror; Morgan thinks you look sort of like a fish, Hardy said you look like a boat. I say you look not unlike our close relatives when they stand up; somewhat shorter arms and longer legs, but similar, and with an awful lot of bits sticking out in rather inconvenient places for streamlining. Not to mention our hair. Who's right, Morgan, Hardy, or me?

[11:00] *And the ironic thing about it is that they are not staving off the aquatic theory to protect a theory of their own, which they've all agreed on, and they love. There is nothing there. [emphatic repeated pointing] They are staving off the aquatic theory to protect a vacuum. (Laughter) (Applause)*

Even if there was no theory whatever out there (which is untrue) it would make sense to "stave off the aquatic theory" because it runs counter to facts and is argued for with falsehoods. Does it makes sense to accept an idea which is counter to facts and argued with falsehoods? Morgan is essentially arguing that it does.

[11:26] *How do they react when I say these things? One very common reaction I've heard about 20 times is, "But it was investigated. They conducted a serious investigation of this at the beginning, when Hardy put forward his article." I don't believe it. For 35 years I've been looking for any evidence of any incident of that kind, and I've concluded, that that's one of the urban myths. It's never been done.*

Funny stuff. Nonsensical, but funny. Morgan is well aware of many such "incidents", including my web site. She's therefore saying something she knows isn't true; there's a word for that.

[12:32] *"And if you've got a scientific problem like that, you can't solve it by holding a head count and say more of us say yes than say no. Apart from that, some of the heads count more than others. Some of them have come over. [gets ready to count on fingers] There was Professor Tobias; he's come over. Daniel Dennett; he's come over. Sir David Attenborough; he's come over. Anybody else*

out there, come on in, the water's lovely. And now we've got to look to the future. Ultimately, one of three things is going to happen. Either they will go on for the next 40 years, 50 years, 60 years saying yeah, well we don't talk about that; let's talk about something interesting. That'll be very sad. The second thing that could happen is that some young genius will arrive and say I've solved it, it was not the savanna, it was not the water, it was this. No sign of that happening either. I don't think there's a third option. So the third thing that might happen is a very beautiful thing. If you look back at the early years of the last century there was a standoff and a lot of bickering and bad feeling between the believers in Mendel and the believers in Darwin. It ended with a new synthesis. Darwin's ideas and Mendel's ideas blending together and I think the same thing will happen here. You get a new synthesis; Hardy's ideas and Darwin's ideas will be blended together and we can go forward from there and really get somewhere. That will be a beautiful thing. It would be very nice for me if it happened soon." [audience laughter and applause]

Tobias' "coming over" statements: "Nowhere have I stated, either in print or on a public platform, or on the media, that I support the AAH!" and "I have also said "I am not yet convinced that the AAH is correctly applied to all of the morphological and functional traits that its proponent have proposed as "aquatic [sic] traits" of the hominids". Dennett's "support" isn't terribly clearcut either. There are three times I know of when Dennett has referenced the aquatic ape idea. The first time he says that in the early 1990s he asked (unnamed) people to come up with an off the cuff detailed refutation of the AAT/H and they couldn't; I've pointed out that they couldn't do that even for well known wacky ideas like books on the Bermuda Triangle, the Philadelphia Experiment, and so on (those were some of the other books by the publisher of Morgan's first books). Then in a BBC Radio program on the AAT/H he offered that he didn't think there'd been "the right sort of measured and sober response; at least if there has been I haven't been able to find it". Well, I'd have to say that Dennett didn't look too hard -- try a Google search, Dan :) But then maybe my site, or Adrienne Zihlman's articles, or the articles in the book *The Aquatic Ape: Fact or Fiction?*, aren't "the right sort". Lately he's written an article where he places the AAT/H in a category of "enticing hypotheses that are actively defended but still insufficiently supported by hard facts", which is hardly a ringing endorsement even before you remember that he's suggesting that ID (the creationism variant "intelligent design") "get in line behind" them.

Attenborough's support *is* more clearcut, but how accurate he is when he gets into it is shown in my review of his BBC Radio 4 show on the subject -- not very.

If I were a praying man every night I'd be on my knees pleading to be delivered from "support" like that.

[14:55] *"So if it's going to come and it's going to happen what's holding it up? I can tell you that in three words: academia says no. They decided in 1960 that belongs with the UFOs and the yetis and it's not easy to change their minds. The professional journals won't touch it with a barge pole, the textbooks don't mention it, the syllabus doesn't even the fact that we're naked let alone look for a reason to it. Horizon, which takes it cue from the academics, won't touch it with a barge pole. So we never hear the case put for it, except in jocular references to people on the lunatic fringe. I don't know quite where this diktat comes from. [holds hand up and brushes down, indicating it's from on high] [looking up] Somebody up there is issuing the commandment Thou shalt not believe in the aquatic theory, and if you hope to make progress in this profession and you do believe it you'd better keep it to yourself cause it will get in your way.*

This is talk that can only come from someone who has not the slightest notion of science, scientists, the history of science. Her claim, longstanding but here made probably more explicit than she has before, that academia works by diktat and edicts, or even that it could, is beyond nonsensical.

[Elaine Morgan's 2009 TED Talk](#)

Aquatic and semiaquatic mammals

I've mentioned elsewhere that AAT/H proponents are typically extremely coy about what aquatic animals we're supposed to resemble; they're also somewhat idiosyncratic about what they say constitutes an "aquatic" or "semiaquatic" mammal. Marc Verhaegen has often claimed that non-aquatic mammals such as rhinoceros and mountain beavers are aquatic, which is nonsense. Others claim pigs are; again nonsense. Many times they don't really like to say at all, although they don't spend much time looking at actual aquatic and semiaquatic mammals, preferring vagueness, merely claiming that humans share hair and fat characteristics with "aquatics" or that such characteristics are found in "an aquatic environment". But if you take the step of looking at what animals they have to be talking about they get very defensive, because if you do you end up thinking "how much like a seal am I really?".

The general AAT/H vagueness about degree of water use has followed this general arc:

1. a fairly vague statement by Hardy (less aquatic than otters for maybe 10 million years);
2. virtually nothing beyond "as aquatic as an otter" or "less aquatic than an otter" for the *next forty years* (except for Hardy in 1978 making his earlier statement far more explicit (now 5-6 hours in water every day for 20 or so

million years); okay, it's completely impossible -- longer than hominids of any kind have existed -- but at least he said what he was thinking);

3. Algis Kuliukas' formulation of the "more aquatic than apes" phrase (sometimes specifically chimps); his definition is "...that water has acted as an agent of selection in the evolution of humans more than it has in the evolution of our ape cousins..." (the full definition is on the page about him on this site). This is so vague that's it's meaningless.

I mean *really*, water has acted as an "agent of selection" in the evolution of *wolves and black bears* more than it has in the evolution of our ape cousins, and wolves and black bears aren't semiaquatic nor were their ancestors.

That can be contrasted with typical scales of aquaticness used by actual zoologists. For instance, when I was recently making a list of semiaquatic and aquatic mammals one source I used had this as a note for a table of aquatic and semiaquatic mammals:

NOTE: Habitat: M, marine; C, coastal; F, freshwater. Degree to which aquatic: 1, obligately aquatic; 2, mostly aquatic, return to land for reproduction and resting; 3, moderately aquatic, fully capable of terrestrial locomotion, may forage on land

From Table 1.4 by Henry Pihlström, "Introduction: On Becoming Aquatic" in *Sensory Evolution on the Threshold: Adaptations in Secondarily Aquatic Vertebrates* edited by J. G. M. Thewissen and Sirpa Nummela
Publisher: University of California Press

Or consider the explanation set forth in the "Methods" section of a 2001 paper by Bininda-Emonds et al.

("Flippers versus feet: comparative trends in aquatic and non-aquatic carnivores", *Journal of Animal Ecology*, 2001, 70:386-400): "We consider aquatic carnivores to be those species in which the aquatic habitat inevitably plays a key role in the life-cycle of an individual..."

Because the AAT/H claims that aquatic and semiaquatic living creates selection for hairlessness and fatness, I thought it would be useful to have a list of all the aquatic and semiaquatic mammals, and I'm also going to look at how many times aquaticism has evolved in aquatic and semiaquatic mammals and how many are hairless and/or fatty. The list totals 203 species; for my purposes it's broken down as follows:

112 hairy; 75 not including seals and walrus

91 hairless (84 cetaceans, 5 sirenia, 2 hippos)

125 "fatties" (84 cetaceans, 5 sirenia, 36 seals and walrus)

35 of the hairy ones tropical (or in the case of 2 or 3 species ranging into the tropics or subtropical areas)

4 of the species listed are now (relatively recently) extinct

The reason I've broken it down that way is the claims I've seen used over the years by AAT/H proponents and what they've said when their claims get critiqued. Two of the basic claims are that natural selection (and pointedly *not* sexual selection or genetic drift or the demands of thermoregulation) and a watery environment is behind humans being "hairless" and relatively fat. (They don't accurately describe human hair and fat characteristics but that's another mark against their ideas.) AAT/H proponents generally do this without saying what aquatic and/or semiaquatic mammals they're talking about; typically they just claim water use does the deed. Typically they also refuse to say how much water use this takes, sometimes going to comical lengths to *not* say. Looking at this list gives you an idea of just what amount of water use it takes to, say, lose body hair.

"Fatties" refers to generally a good deal more body fat than is "average" among wild mammals such as primates, bovidae, most carnivores, and such; the average more or less seems to be around 5-10%. Naturally this varies a lot seasonally and with the health of the animal and how its food supply is, how much predation pressure they face, etc. so it is approximate, but we're looking for year-round "fatties" here. For this purpose we're also ignoring the many fatty non-semiaquatic mammals -- such as badgers, bears, pigs, camels, and hedgehogs -- just as AAT/H proponents virtually always do.

When you point out that semiaquatic mammals are often hairy, AAT/H proponents sometimes claim that of course our early ancestors were tropical and only cold weather semiaquatic mammals are hairy. This is not true, so to point this out in the full list I've marked the tropical semiaquatic mammals, or those that range into the tropics or subtropics. Contrary to the common AAT/H claim, except for cetaceans, sirenia, and hippos they're all hairy.

How many times has aquaticism or semiaquaticism arisen among extant mammals?

I've got the full list of all aquatic and semiaquatic mammal species at the bottom of this page, and it's pretty long, but first there's the question of how many times hairlessness evolved among aquatic and semiaquatic mammals. Since AAT/H proponents typically do their best to ignore hairy aquatic and semiaquatic mammals entirely they never really get around to this question, but it's interesting. What

makes it interesting is that there are two reasons the AAT/H claim that water use begets hairlessness falls down.

The first is the degree of water use; even if you assume that water use rather than body size and thermoregulation is behind all these species' hairlessness you have only species which have been aquatic for tens of millions of years and which are highly aquatic, except for the hippos. With hippos it's generally acknowledged that the well known physics of body volume vs. surface is behind their hairlessness. For those who aren't aware of how this works, it's the same principle that a radiator works on; all those fins on a radiator dissipate heat because there's more surface area to volume. Large animals with stocky bodies tend to retain heat, and they're helped by losing that hair. Examples are hippos, rhinos, and elephants. Further evidence that this is why they're relatively hairless is the ice age far north versions of the elephant and the rhino, the woolly mammoth and the woolly rhinoceros. Of course cetaceans and sirenians are also almost all very large and have relatively small limbs in the form of flippers; they'd have a big problem with getting rid of heat if they weren't in the water virtually all the time, and in fact when these animals are transported or beached a major problem for them is overheating. Whether their ancestors lost body hair because of swimming (which seems especially unlikely in the case of slow-moving sirenians) or just body size is not known.

The other question is how commonly hairlessness is connected with aquaticism at all. Even if you just add up how many aquatic and semiaquatic mammal species are hairless, the total is less than half. But it's just not possible that each and every present-day aquatic and semiaquatic mammal independently evolved aquaticness. We know that many of them have aquatic ancestors and have evolved from a long line of aquatic ancestors, and this is especially true for those which are hairless; it's incredibly unlikely that their aquatic ancestors were hairy for tens of millions of aquatic years and then after diverging they all became hairless. And of course even if that incredibly unlikely scenario were the case it would hurt rather than help the AAT/H argument. So the question is how many times has aquaticness evolved, and in the context of the AAT/H claims, how many times for hairless mammals and how many times for hairy mammals.

So here's the answer:

Almost certainly no more than 3 times for hairless mammals as opposed to almost certainly *at least* 29 times for hairy mammals.

This means, contrary to the common AAT/H claim, comparisons with other mammals do not support the idea that living a semiaquatic lifestyle likely to make you "hairless". It's actually rare that hairlessness and aquaticism are correlated. The AAT/H claim already falters badly just by the existence of many hairy semiaquatic mammals. It falters even more from the existence of hairy tropical semiaquatic mammals. It falters even more when you see that among aquatic and semiaquatic mammals the *only* hairless ones are either very large and almost certainly hairless due to the physics of thermoregulation, or are fully aquatic and

have been for tens of millions of years, longer than hominids of any sort have existed. Or they are both very large *and* fully aquatic for tens of millions of years. When you then go further than merely counting the hairy and non-hairy species and instead look at how many times aquaticism arose in mammals, and you separate that list into hairy and not hairy, it's just devastating. No wonder none of the idea's proponents has done this obvious test.

Another AAT/H claim brought down by examining relatively easy to find facts is the idea that water makes mammals "fatties". This is because the aquatic "fatties" are all cetaceans, sirenia, and pinnipeds, all aquatic for tens of millions of years.

I've had to make some educated guesses regarding some of the hairy semiaquatic mammal groups and whether closely related mammals diverged before or after becoming semiaquatic, and whenever this was necessary I decided to help the AAT/H case by erring on the side of assuming fewer cases of semiaquaticism evolving in mammals which remain hairy. For instance the European and North American minks are different genres, but it seems likely enough they diverged after becoming semiaquatic. I've lumped them together as one instance of semiaquaticism arising. The muskrat and the round-tailed muskrat, despite the common names, are fairly different animals, and it seems likely enough they did evolve semiaquaticism separately, but they *are* the closest related among the Arvicola and in this list I assume the opposite: that they diverged after becoming semiaquatic, so they too are also lumped together as one instance of semiaquaticism arising. There are notes after those groups of species I've done this for.

Here's the lists which show how often aquaticism and semiaquaticism arose in mammals; each instance of aquaticism or semiaquaticism arising is marked with a "+" preceding the species or group of species:

Non-hairy Aquatic or Semiaquatic Mammals

Aquaticness evolved *very* probably no more than 3 times, as each group branched out from a mammal which had habits etc. very much like the present-day mammals in the group and all mammals in the group are similar in habits etc.

+ Cetaceans (whales, dolphins, porpoises)

+ Sirenia (manatees, dugongs, sea cows)

+ Hippopotamidae (hippopotamus)

Hairy Aquatic or Semiaquatic Mammals

Aquaticness evolved *at least* 29 times -- marked "+" if the group has an evolutionary past distinct from others in the list and/or has closely related members which are not semiaquatic, esp. if a majority of the members of the group are not semiaquatic.

In those groups marked with a note there is some possibility that semiaquaticness evolved separately in the groups mentioned but for the sake of the estimate of how many times semiaquaticness arose I've assumed they did not; this makes the possible error on the side of fewer cases of semiaquaticism evolving in mammals which remain hairy.

Monotremata

+ Ornithorhynchidae (platypus)

Didelphidae

+ Didelphinae (water opossum)

Soricomorpha

Soricidae (shrews)

+ Soricinae (red-toothed shrews)

Nectogale (elegant water shrew)

Neomys (Eurasian water shrew)

Nectogalini (Old World water shrews)

Chimarrogale (Asiatic water shrews)

note: it's possible but certainly far from definite that Nectogale, Neomys, and Nectogalini diverged from a semiaquatic ancestor; they're all in Nectogalini but so are many non-semiaquatic shrews

Tenrecidae

+ Potamogalinae (otter shrews)

+ Limnogale (aquatic tenrec)

Talpidae

+ Talpinae (Pyrenean desman)

+ Scalopinae (star-nosed mole)

Carnivora

Canidae

Caniformia

+ Speothos (bush dog)

Mustelidae

+ Lutrinae (otters)

+ Neovison (North American minks)

+ Mustela (European mink)

note: I had previously listed Neovision and Mustela as one instance of becoming aquatic, given what I thought was the possibility that North American and European minks may have diverged after becoming semiaquatic. I've learned since from Henry Pihlström (personal communication) that molecular data shows they "are not particularly closely related; they belong to independently evolved semiaquatic lineages", and that in fact based on MtDNA "nowadays many authors feel that the American minks need to be separated into a different genus" (Henry Pihlström, personal communication).

Ursidae

+ Ursus maritimus (polar bear)

Viverridae (civets, genets)

Viverrinae

+ Genetta; aka Osbornictis (aquatic genet)

Hemigalinae

+ Cynogale (otter civet)

Herpestidae (mongoose)

+ Atilax (marsh mongoose)

+ Pinnipeds (seals and walrus)

Artiodactyla

Bovidae

+ Reduncinae (lechwe)

+ Bovinae (sitatunga)

Rodentia

Hystricomorpha

+ Myocastoridae (nutria)

+ Caviidae (capybara)

+ Thryonomyidae (cane rats)

Castorimorpha

+ Castoridae (beavers)

Myomorpha

Cricetidae

Arvicolinae

+

Arvicola (water voles)

+

Ondatra (muskrat)

Neofiber (round-tailed muskrat)

note: Ondatra and Neofiber may have evolved aquaticness separately but they *are* the closest related among the Arvicola; this list assumes they diverged after becoming semiaquatic which may be incorrect

Sigmodontinae

+

Neusticomys (fish-eating rats)

Ichthyomys (crab-eating rats)

Antomys (fish-eating rat)

Rheomys (Central American water mice)

note: it's possible Neusticomys, Ichthyomys, Antomys, and Rheomys diverged from a semiaquatic ancestor

+

Chibchanomys (water mice)

Holochilus (the web-footed rats)

Nectomys (the Neotropical water rats)

Oryzomys (marsh rice rat)

Amphinectomys (Ucayali water rat)

note: it's possible but certainly far from definite that Chibchanomys, Holochilus, Nectomys, Oryzomys, and Amphinectomys all diverged from a semiaquatic ancestor; they're all in Ichthyomyini but so are many non-semiaquatic rodents

+

Scapteromys

Muridae

Murinae

+

Colomys (African water rat)

+

Nilopegamys (amphibious rat)

Full list of aquatic and semi-aquatic mammals

Compiled from [ICUN Red List](#) and [ADW](#) (Animal Diversity Web, University of Michigan Museum of Zoology, plus extras from Table 1.4 by Henry Pihlström, ["Introduction: On Becoming Aquatic"](#), in *Sensory Evolution on the Threshold: Adaptations in Secondarily Aquatic Vertebrates* edited by J. G. M. Thewissen and Sirpa Nummela, Feb. 2008, University of California Press

List totals 203 species

Note: * = tropical (or somewhat, with explanation -- ie. range extends into tropical or subtropical regions)

Carnivora

Polar bear (*Ursus maritimus*)

Sea otter (*Enhydra lutris*)

Marine otter (*Lontra felina*)

Eurasian otter (*Lutra lutra*)

Hairy-nosed otter (*Lutra sumatrana*) *

Spotted-necked otter (*Hydriclis maculicollis*) *

Smooth-coated otter (*Lutrogale perspicillata*) *

North American River Otter (*Lontra canadensis*) * (ranges from far north down to Gulf coast to the southern tip of Florida)

Southern river otter (*Lontra provocax*)

Neotropical river otter (*Lontra longicaudis*) *

Giant otter (*Pteronura brasiliensis*) *

African clawless otter (*Aonyx capensis*) *

Oriental small-clawed otter (*Aonyx cinerea*) *

American mink (*Neovison vison*) * (ranges from far north down to Gulf coast to the southern tip of Florida)

Sea mink (*Neovison macrodon*) status: extinct since 1894, northeastern North America, hunted for fur

European mink (*Mustela lutreola*)

Otter Civet (*Cynogale bennettii*) *

Bush dog (*Speothos venaticus*) * (see notes at bottom of list)

Aquatic genet (*Genetta piscivora*) *

Marsh mongoose (*Atilax paludinosus*) *

Bovidae

Kafue Lechwe (*Kobus leche kafuensis Haltenorth*) (antelope) *

Sitatunga (*Tragelaphus spekii*) (antelope) *

Monotremes

Platypus (*Ornitho rynchus anatinus*)

Didelphimorphia (American marsupials)

Water Opossum (*Chironectes minimus*) * (can also be found at high elevations)

Soricomorpha

Star-nosed mole (*Condylura cristata*)

Water shrew (*Sorex palustris*)

Pacific water shrew (*Sorex bendirii*)

Pyrenean Desman (*Galemys pyrenaicus*)

Aquatic Tenrec (*Limnogale mergulus*)

Nimba otter-shrew (*Micropotamogale lamottei*) *

Rwenzori Otter Shrew (*Micropotamogale ruwenzorii*)

Giant Otter Shrew (*Potamogale velox*) *

Sumatran Water Shrew (*Chimarrogale sumatrana*) *

Malayan Water Shrew (*Chimarrogale hantu*) *
Himalayan Water Shrew, Elegant Water Shrew (*Chimarrogale himalayica*)
Bornean Water Shrew (*Chimarrogale phaeura*) *
Japanese Water Shrew (*Chimarrogale platycephalus*)
Chinese Water Shrew (*Chimarrogale styani*)
Elegant Water Shrew (*Nectogale elegans*)
Transcaucasian Water Shrew (*Neomys teres*)
Mediterranean Water Shrew (*Neomys anomalus*)
Eurasian Water Shrew (*Neomys fodiens*)

Rodentia

Capybara (*Hydrochoerus hydrochaeris*) *
American Beaver (*Castor canadensis*)
European beaver (*Castor fiber*)
Nutria or coypu (*Myocastor coypus*)
Greater cane rat (*Thryonomys swinderianus*) *
Peruvian Fish-eating Rat (*Neusticomys peruviansis*)
Venezuelan Fish-eating Rat (*Neusticomys venezuelae*) *
Montane Fish-eating Rat (*Neusticomys monticolus*)
Musso's Fish-eating Rat (*Neusticomys mussoi*)
Oyapock's Fish-eating Rat (*Neusticomys oyapocki*) *
Tweedy's Crab-eating Rat (*Ichthyomys tweedii*)
Crab-eating Rat (*Ichthyomys hydrobates*)
Pittier's Crab-eating Rat (*Ichthyomys pittieri*)
Stolzmann's Crab-eating Rat (*Ichthyomys stolzmanni*)
Ecuador fish-eating rat (*Anotomys leander*)
Mexican water mouse (*Rheomys mexicanus*)
Goldman's water mouse (*Rheomys raptor*) -- may be slightly less aquatic than other *Rheomys*
Underwood's water mouse (*Rheomys underwoodi*)
Las Cajas ichthyomyine (*Chibchanomys orcesi*)
Chibchan water mouse (*Chibchanomys trichotis*)
Web-footed Marsh Rat (*Holochilus brasiliensis*) * (wide-range)
Chaco Marsh Rat (*Holochilus chacarius*)
South American Water Rat (*Nectomys squamipes*) *
Trinidad Water Rat (*Nectomys palmipes*) *
Magdalena Nectomys (*Nectomys magdalenae*) *
Western Amazonian Nectomys (*Nectomys apicalis*) *
Swamp Rat (*Scapteromys tumidus*)
Argentine swamp rat (*Scapteromys aquaticus*)
Australian water rat (*Hydromys chrysogaster*)
Earless Water Rat (*Crossomys moncktoni*)
Eurasian Water Vole (*Arvicola amphibius*)

Southern Water Vole (*Arvicola sapidus*)
Water Vole (*Microtus richardsoni*)
Round-tailed muskrat (*Neofiber alleni*) * (south Florida)
Muskrat (*Ondatra zibethicus*)
Ucayali water rat (*Amphinectomys savamis*) *
Alfaro's Rice Water Rat (*Sigmodontomys alfari*) *
Marsh Rice Rat (*Oryzomys palustris*) (Gulf coast and to southern tip of Florida)
African water rat (*Colomys goslingi*) *
Amphibious rat (*Nilopegamys plumbeus*)

Hippopotamidae

Hippopotamus (*Hippopotamus amphibius*) *
Pygmy hippopotamus (*Choeropsis liberiensis*) *

Pinnipeds (seals and walrus)

Crabeater Seal (*Lobodon carcinophaga*)
Bearded Seal (*Erignathus barbatus*)
Hooded Seal (*Cystophora cristata*)
Northern Fur Seal (*Callorhinus ursinus*)
South American Fur Seal (*Arctocephalus australis*)
New Zealand Fur Seal (*Arctocephalus forsteri*)
Galápagos Fur Seal (*Arctocephalus galapagoensis*) *
Antarctic Fur Seal (*Arctocephalus gazella*)
Juan Fernández Fur Seal (*Arctocephalus philippii*)
Afro-Australian Fur Seal (*Arctocephalus pusillus*)
Guadalupe Fur Seal (*Arctocephalus townsendi*) * (along Baja Mexico coastline)
Subantarctic Fur Seal (*Arctocephalus tropicalis*)
Caspian Seal (*Pusa caspica*)
Ringed Seal (*Pusa hispida*)
Baikal Seal (*Pusa sibirica*)
Spotted Seal (*Phoca largha*)
Harbour Seal (*Phoca vitulina*)
Ross Seal (*Ommatophoca rossii*)
Harp Seal (*Pagophilus groenlandicus*)
New Zealand Sea Lion (*Phocarctos hookeri*)
Australian Sea Lion (*Neophoca cinerea*)
Californian Sea Lion (*Zalophus californianus*)
Japanese Sea Lion (*Zalophus japonicus*) status: extinct
South American Sea Lion (*Otaria flavescens*)
Galápagos Sea Lion (*Zalophus wolfebaeki*)
Steller Sea Lion (*Eumetopias jubatus*)
Northern Elephant Seal (*Mirounga angustirostris*)
Southern Elephant Seal (*Mirounga leonina*)

Weddell Seal (*Leptonychotes weddellii*)
Grey Seal (*Halichoerus grypus*)
Ribbon Seal (*Histiophoca fasciata*)
Leopard Seal (*Hydrurga leptonyx*)
Mediterranean Monk Seal (*Monachus monachus*) * (Mediterranean coast, Europe and northern Africa)
Hawaiian Monk Seal (*Monachus schauinslandi*) *
Caribbean Monk Seal (*Monachus tropicalis*) status: extinct *
Walrus (*Odobenus rosmarus*)

Cetaceans (whales, dolphins, and porpoises)

Bowhead Whale (*Balaena mysticetus*)
Common Minke Whale (*Balaenoptera acutorostrata*)
Antarctic Minke Whale (*Balaenoptera bonaerensis*)
Sei Whale (*Balaenoptera borealis*)
Bryde's Whale (*Balaenoptera edeni*)
Blue Whale (*Balaenoptera musculus*)
Omura's Whale (*Balaenoptera omurai*)
Fin Whale (*Balaenoptera physalus*)
Arnoux's Beaked Whale (*Berardius arnuxii*)
Baird's Beaked Whale (*Berardius bairdii*)
Pygmy Right Whale (*Caperea marginata*)
Commerson's Dolphin (*Cephalorhynchus commersonii*)
Chilean Dolphin (*Cephalorhynchus eutropia*)
Heaviside's Dolphin (*Cephalorhynchus heavisidii*)
Hector's Dolphin (*Cephalorhynchus hectori*)
Beluga (*Delphinapterus leucas*)
Long-beaked Common Dolphin (*Delphinus capensis*)
Short-beaked Common Dolphin (*Delphinus delphis*)
Southern Right Whale (*Eubalaena australis*)
North Atlantic Right Whale (*Eubalaena glacialis*)
North Pacific Right Whale (*Eubalaena japonica*)
Pygmy Killer Whale (*Feresa attenuata*)
Short-finned Pilot Whale (*Globicephala macrorhynchus*)
Long-finned Pilot Whale (*Globicephala melas*)
Risso's Dolphin (*Grampus griseus*)
North Atlantic Bottlenose Whale (*Hyperoodon ampullatus*)
Southern Bottlenose Whale (*Hyperoodon planifrons*)
Indo-pacific Beaked Whale (*Indopacetus pacificus*)
Boto (*Inia geoffrensis*)
Pygmy Sperm Whale (*Kogia breviceps*)
Dwarf Sperm Whale (*Kogia sima*)
Fraser's Dolphin (*Lagenodelphis hosei*)

Atlantic White-sided Dolphin (*Lagenorhynchus acutus*)
White-beaked Dolphin (*Lagenorhynchus albirostris*)
Peale's Dolphin (*Lagenorhynchus australis*)
Hourglass Dolphin (*Lagenorhynchus cruciger*)
Pacific White-sided Dolphin (*Lagenorhynchus obliquidens*)
Dusky Dolphin (*Lagenorhynchus obscurus*)
Baiji (*Lipotes vexillifer*)
Northern Right Whale Dolphin (*Lissodelphis borealis*)
Southern Right Whale Dolphin (*Lissodelphis peronii*)
Humpback Whale (*Megaptera novaeangliae*)
Sowerby's Beaked Whale (*Mesoplodon bidens*)
Andrew's Beaked Whale (*Mesoplodon bowdoini*)
Hubbs' Beaked Whale (*Mesoplodon carlhubbsi*)
Blainville's Beaked Whale (*Mesoplodon densirostris*)
Gervais' Beaked Whale (*Mesoplodon europaeus*)
Ginkgo-toothed Beaked Whale (*Mesoplodon ginkgodens*)
Gray's Beaked Whale (*Mesoplodon grayi*)
Hector's Beaked Whale (*Mesoplodon hectori*)
Strap-toothed Whale (*Mesoplodon layardii*)
True's Beaked Whale (*Mesoplodon mirus*)
Perrin's Beaked Whale (*Mesoplodon perrini*)
Pygmy Beaked Whale (*Mesoplodon peruvianus*)
Stejneger's Beaked Whale (*Mesoplodon stejnegeri*)
Spade-toothed Whale (*Mesoplodon traversii*)
Narwhal (*Monodon monoceros*)
Melon-headed Whale (*Peponocephala electra*)
Sperm Whale (*Physeter macrocephalus*)
Finless Porpoise (*Neophocaena phocaenoides*)
Irrawaddy Dolphin (*Orcaella brevirostris*)
Australian Snubfin Dolphin (*Orcaella heinsohni*)
Killer Whale (*Orcinus orca*)
Spectacled Porpoise (*Phocoena dioptrica*)
Harbour Porpoise (*Phocoena phocoena*)
Vaquita (*Phocoena sinus*)
Burmeister's Porpoise (*Phocoena spinipinnis*)
White-flanked Porpoise (*Phocoenoides dalli*)
South Asian River Dolphin (*Platanista gangetica*)
Franciscana (*Pontoporia blainvillei*)
False Killer Whale (*Pseudorca crassidens*)
Guianian River Dolphin (*Sotalia fluviatilis*)
Indo-Pacific Humpbacked Dolphin (*Sousa chinensis*)
Atlantic Humpbacked Dolphin (*Sousa teuszii*)

Pantropical Spotted Dolphin (*Stenella attenuata*)
Clymene Dolphin (*Stenella clymene*)
Striped Dolphin (*Stenella coeruleoalba*)
Atlantic Spotted Dolphin (*Stenella frontalis*)
Spinner Dolphin (*Stenella longirostris*)
Rough-toothed Dolphin (*Steno bredanensis*)
Shepherd's Beaked Whale (*Tasmacetus shepherdi*)
Indo-pacific Bottlenose Dolphin (*Tursiops aduncus*)
Common Bottlenose Dolphin (*Tursiops truncatus*)
Cuvier's Beaked Whale (*Ziphius cavirostris*)

Sirenia

South American Manatee (*Trichechus inunguis*)
West Indian Manatee (*Trichechus manatus*)
West African Manatee (*Trichechus senegalensis*)
Dugong (*Dugong dugon*)
Steller's Sea Cow (*Hydrodamalis gigas*) status: extinct

Others not aquatic enough to be called semi-aquatic, like fishing cat; proboscis monkey; several species of mouse deer which regularly dive into water and "swim" (or rather walk) underwater, remaining there for some time (plus/minus 1 hour, albeit coming up for air during that time, breathholding for 3-4 minutes at a time) to escape predators (Greater Oriental Chevrotain *Tragulus napu*, White-spotted Chevrotain *Moschiola meminna*, and Water Chevrotain *Hyemoschus aquaticus*), they remain underwater for some time; tapirs; babyrussa (not all species of babyrussa are relatively hairless as is commonly thought); swamp rabbits and marsh rabbits (both of which are species of North American cottontail); pacas (2 species, which often try to escape predators by swimming)

Bush dog notes: "[It is adapted to a semi-aquatic life amongst the forest.](#)"; "[In captivity Speothos has been found to enter water readily and to swim well -- 'with otter-like ease', according to Hershkovitz \(1969\)](#)"; "[They are semiaquatic and can 'dive and swim underwater with great facility'. \(Nowak, 1999, Walker's Mammals of the World.](#)" ICUN does not call this species semiaquatic but besides the above several academic papers also point out that it *is* semiaquatic.

Danakil Island and the Baboon Marker: two of the AAT/H's "false facts" which have indeed "endured long"

There are some AAT/H arguments which you just don't see much, if ever, anymore from the principals, but you do see them pop up again and again when people ask about the idea. This is because they've become those "false facts" Darwin warned about, and they do, as he said, "endure long". Long after the principals in the

pro-AAT/H camp have dropped them like a hot potato, they come back over and over. You may have seen one of these in a forum or newsgroup, and heard someone mention one as evidence for the AAT/H, and wonder why you don't hear a refutation of it -- you may wonder even more why AAT/H proponents rarely, if ever, mention them now. Interestingly enough, they generally weren't dropped, as far as I can see, because any AAT/H proponent disproved them, or even accepted that they aren't good evidence. They've just quietly been disappeared, like Soviet officials on the reviewing stand of a May Day parade, or the unwanted ex in old photos. Like the Cheshire cat, only the smile remains to show they were ever there.

Danakil Island

Now there's a blast from the past for Morgan readers, and it still pops up even though Morgan herself has for whatever reason made a point of not mentioning it for years. This is the place where, supposedly, Morgan's aquatic apes were busy evolving while the other apes slept. It seems, according to Morgan's scenario of the time, that this population of ever-more aquatic early hominins were cut off from the rest of the population because -- I kid you not -- they were trapped on a near offshore island!

That's right -- on an island.

At that time Morgan was very big on the idea of a marine-dwelling ancestor and got this idea of their being isolated on what she described as a close offshore island near Afar, the region where the earliest hominids known then had been found. The Danakil Alps, a mountain range near the sea there, had, she described, been partially submerged and the bits sticking up constituted "Danakil Island". This idea came about as the brainchild of Leo P. La Lumiere, who was an acoustic engineer (but who Morgan has repeatedly and inaccurately claimed was a geologist), who was in vogue for a time with AAT/H proponents; he had no other connection to the AAT/H other than Morgan's championing his idea of Danakil Island, so when it was left to drift on the seas of memory, so was he. Since the island tends to sail back whenever people ask about Morgan's books, so should La Lumiere.

La Lumiere didn't do the primary research about the Danakil past -- nothing wrong with that, as the libraries of the world have loads of info that really ought to be used. The information LaLumiere used regarding the Afar region came mostly from two western Ontario researchers who studied the past shorelines of northeastern Africa. There was a period when the area of Afar was semi-submerged, but the claims of La Lumiere and Morgan do not follow from that fact.

There are several problems with this idea. One was Morgan's claim at the time that we evolved in a saltwater environment and she had a list of reasons why, all of

which I pointed out were wrong (when we did the back in forth in early 1990s newsgroups) and which are covered on other pages on my website. And, of course, it never seemed sensible that a population of ever-more aquatic hominids were trapped on an near offshore island for any great length of time, much less the millions of years Morgan claimed for this supposed episode.

The last is that Danakil "island" was never actually an island; it was a peninsula with one end covered by flood basalt, which are low viscosity lava flows. Now La Lumiere seemed to think (and Morgan and others followed his lead) that this meant it was a continual sheet of solid rock many miles wide and created an insurmountable barrier to those hominids. But flood basalts don't just remain rock; for instance the Columbia River valley east of Portland Oregon is flood basalts; the Massif Central in France is another example. These basalts break down and things grow on them, just as things grow on lava flows in general, and in fact they grow pretty darned well. It takes a while, but you're not cutting off a group of hominids for several million years, as La Lumiere and Morgan thought, with flood basalts.

La Lumiere simply did not know what he was talking about, and Morgan made no apparent attempt to learn more than she had from La Lumiere (if she had she wouldn't have touted the idea). Not a good way to do science, in fact not a possible way to do science. And certainly not a good way to demonstrate your research skills and realilbity.

I also point out in my page critiquing Elaine Morgan's 2008 book, *The Naked Darwinist*, that she brings in La Lumiere in a truly bizarre claim: Morgan falsely claimed that up until La Lumiere wrote up his idea the oldest hominid fossils had been found in South Africa and that La Lumiere had pointed science to the rich and important new hominid fossil area in the Afar region. This could only be true if La Lumiere had utilized time travel (if only he'd shared it with the world :), because of course La Lumiere wrote up his idea in 1981 and Donald Johanson took over the Afar fossil site well before that (Lucy was discovered in 1974 and that was the third year Johanson had been looking for hominid fossils there) and the Leakeys had been finding what were then very early hominids in Kenya for 20 years before that. The idea Morgan here promotes, that La Lumiere pointed a fossil area not already well known, is sheer nonsense and easily seen to be nonsense by any researcher who had done even the most cursory study of human evolution, like reading a couple popular books -- for instance the bestseller *Lucy: The Beginnings of Humankind*, by Johanson, which Morgan had indeed read.

The Baboon Marker

This was tied to Morgan's use of Danakil Island; she was looking for something that said our ancestors were not in Africa when all the known fossils were, and the "baboon marker gene" came along just at the right time.

What this marker supposedly showed was that all African primates had a marker gene showing they'd been exposed to a virus that had coursed through the African baboon population several million years ago. All but humans. This meant (again, supposedly) that humans' ancestors had not been in Africa at that crucial period of their evolution. The original authors of the study said that meant humans' ancestors had actually come from Asia, while Morgan seized on this to say they'd been trapped on that off-shore island -- swimming, diving, evolving features like seals, otters, and whales; but somehow unable to swim to the mainland a few miles away.

The mid 1970s was the time of molecular data's rise as really important evidence in evolution. The baboon marker gene idea first came out in two papers in the journal *Nature* (in 1974 and 1976) and the conclusions of the authors conflicted with all known fossil data, comparative anatomy data, and virtually all other molecular data, blood data, etc. At the time this created a lot of debate over this idea. Now there isn't any. Why? Perhaps that Great Paleoanthropological Conspiracy that keeps out heretic ideas from the pristine Ivory Tower?

Well, no. Since the baboon marker idea did conflict with all known fossil, comparative anatomy, and virtually all other molecular and blood data, that should make you wonder what's up, and it did for paleoanthropologists at the time. Fortunately, you don't have to wonder too long to see what the problem was.

There was, in the professional anthropological literature, a deal of discussion about this finding when it first came out in 1974 and subsequent years, and there's a very good reason -- besides the conflict with all other, extensive, evidence -- it hasn't been taken as evidence of hominids not being on mainland Africa at the time the baboon virus arose. That is that the researchers who did the work originally did find that humans do carry a marker for the p30 protein of the virus in question, showing that they had indeed been exposed to it (this was also confirmed by other papers on the subject by various researchers). The researchers who did the initial work on this (Benveniste and Todaro) drew the unwarranted conclusion that hominids had not been in Africa at that time -- by ignoring that part of their own findings, which really is rather foolish. Elaine Morgan and later other AAT/H proponents then committed the same error.

In addition, the relative expression of this gene shows a pretty direct correlation with how closely related the various African primates are to baboons, with humans and apes having the least and humans the least of all; this strongly suggests that the expression of the gene is changed over genetic distance, which is what one

might expect. There's also the issue of reexposure; humans during later evolution didn't all live in Africa (as other African primates did) and so the overall population might well be expected to show less reexposure to the virus; this would also aid in lessening the expression of the gene, and would also help produce the results we see.

In the sci.anthropology.paleo newsgroup on 15 Nov 1994 Philip Bigelow explains what this means:

That humans, themselves, show exposure to the baboon type-C virus, although in a very small way, and that the RELATIVE degree of expression of this genetic marker follows the evolutionary family tree of primates all the way up to humans; humans have least expression of the marker (although they have some) and chimps and gorillas have more expression of the marker, and the monkeys and baboon has the greatest expression of the marker: *This is exactly what would be expected from a diminution of the expression of the gene as one goes up the primate family tree.*

And more directly to the point vis a vis the AAT/H (Philip Bigelow in sci.anthropology.paleo again on 15 Nov 1994):

All it takes to disprove Morgan's evidence in this case is AT LEAST SOME genetic marker in humans against this baboon C virus. The authors showed that some exposure MUST have occurred in humans in the distant prehistoric past.

That humans *were* exposed to this virus is also shown by the fact that only some few Africa-descended cats also bear this marker, and those that do are precisely those cats which had the most contact with humans over the years (ie., the domestic cat and some of its wild relatives, the European wildcat and the small African cats found in the Mediterranean region -- cats, during domestication, had a deal of contact with their wild relatives in Europe, Africa, and far West Asia). Since these are the only cats with that marker, it is virtually certain that they got it through contact with humans, and that adds more evidence (besides the p30 protein) that hominids were exposed to the virus. The "baboon marker gene" is not an indicator of hominids not being in mainland Africa during the period the baboon virus arose.

Since the information about the p30 gene and the cats was available in the sources Morgan used to make her case *does* show she did a poor job of research on this issue, apparently just grasping at the straw offered by a poorly thought-out conclusion without really thinking at all about the data. Mind you, I think

Benveniste and Todaro should've done a better job in formulating their conclusion (since it didn't actually follow from their data) but Morgan was remiss in not looking at the data before grabbing a conclusion she liked the sound of. As Philip Bigelow said, she used a conclusion as a "fact", when *a conclusion is an interpretation and should not be used as if it were a fact*; what she needed to use were the facts in the paper (such as the info on the p30 gene and the cats) which in fact *did not* support her position. We've seen this in other AAT/H mistakes, using the logical fallacy called Irrelevant Conclusion (ignoratio elenchi), which Stephen's Guide to the Logical Fallacies defines as: *An argument which purports to prove one thing instead proves a different conclusion.*

References:

Raoul E. Benveniste and George J. Todaro 6 December 1974 "Evolution of C-type viral genes: inheritance of exogenously acquired viral genes". *Nature* 252:456-459.

Raoul E. Benveniste and George J. Todaro 13 May 1976 "Evolution of type-C viral genes: evidence for an Asian origin of man". *Nature* 261:101-108.

References

These aren't well formatted yet; it may be a while before I can put them all in proper order and consistent in layout. Sorry, but at least they're here in one spot, in alphabetical order according to the lead author's name. The first section is the items mentioned on this site; the second section consists of other refs which were also used in constructing this site. This is still not a *complete* set of all the references I used in creating this site, and I still have to tie some of the facts on the site to specific refs. That will be part of my continuing updates.

Alcala, Angel C. (Professor of Biology and Director, Marine Laboratory, Silliman University, Philippines) and Maria Teresa S. Dy-Liacco (Research Assistant, Marine Laboratory, Silliman University, Philippines), 1989 "Behavior and Environment: Habitats". *Crocodiles and Alligators*, Consulting Editor, Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) Facts on File: New York and Oxford.

Bartholomew and Wilkie, 1956 "Body Temperature in Northern Fur Seals". *Journal of Mammalogy* 37:327-337.

Beck, Patrick, [translation of excerpts from Max Westenhöfer's work](#)

Big Zoo

[on chimp swimming ability](#)

Bininda-Emonds, Olaf R.P.; John I. Gittleman and Colleen K. Kelly

2001 "Flippers versus feet: comparative trends in aquatic and non-aquatic carnivores", *Journal of Animal Ecology*, 70:386-400

Black's Medical Encyclopedia

Bonner, W. Nigel, 1990 *The Natural History of Seals*. Facts on File: New York, Oxford, and Sydney.

Bove, Alfred A., M.D. (Chief, Section of Cardiology, Temple University Hospital, Philadelphia, Pennsylvania) and Jefferson C. Davis, M.D. (Hyperbaric Medicine, P.A., San Antonio, Texas), 1990 *Diving Medicine*, Second Edition. W.B. Saunders Company: Philadelphia, London, Toronto, Montreal, Sydney, Tokyo.

Buffetaut, Eric (Director of Research, Centre National de la Recherche Scientifique, University of Paris, France), 1989 "Evolution and Biology: Evolution". *Crocodiles and Alligators*, Consulting Editor, Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) Facts on File: New York and Oxford.

Byrne, Richard W.; and Jennifer M. Byrne, 1988, "Leopard Killers of Mahale", pp. 22-26, in *Natural History*, volume 3.

Campagno, Leonard J.V. (Senior Research Scientist, J.L.B. Smith Institute of Ichthyology, Grahamstown, South Africa), 1987 "Shark Attack in South Africa". *Sharks*. Consulting Editor, Dr. John D. Stevens (Senior Research Scientist, Division of Fisheries, CSIRO Marine Laboratories, Hobart, Tasmania, Australia) Facts on File: New York and Oxford.

Carroll, Robert L., 1988 *Vertebrate Paleontology and Evolution*. (W.H. Freeman and Company: New York)

Chanin, Paul, 1985 *The Natural History of Sea Otters*. Croom Helm: London and Sydney.

Cole, John, 1980 "'Cult Archaeology", *Early Man* Spring 1980

Cole, Monica M., (Department of Geography, Royal Holloway and Bedford New College, University of London), 1986 *The Savannahs: Biogeography and Geobotany*. Academic Press: London.

Colorado State University's Hypertexts for Biomedical Sciences
(<http://arbl.cvmbs.colostate.edu/hbooks/pathphys/>)

Cordain, Loren; Janette Brand Miller, S Boyd Eaton, and Neil Mann
2000 *American Journal of Clinical Nutrition*, Vol. 72, No. 6, 1585-1586, December 2000

Costill, David L.; Ernest W. Maglischo, and Allen B. Richardson, 1992, *Handbook of Sports Medicine and Science: Swimming*. Blackwell Scientific Publications: London.

Darwin, Charles, 1859 (first edition), *On the Origin of Species* (quotes on this site from Project Gutenberg's copy of the 6th edition)

Darwin, Charles, 1871, *Descent of Man, and Selection in relation to Sex*. (quotes on this site from Project Gutenberg's copy of the 1874 2nd edition)

K. D'Août, E. Vereecke, K. Schoonaert, D. De Clercq, L. Van Elsacker and P. Aerts
2004 "Locomotion in bonobos (*Pan paniscus*): differences and similarities between bipedal and quadrupedal terrestrial walking, and a comparison with other locomotor modes" in *J. Anat.* 204, pp353-361.

Deitiker, Philip

Sep 21 2004 newsgroup post ["If you put discovery of the mechanism of evolution first..."](#)

Denton, Derek, 1982 *The Hunger for Salt*. Springer-Verlag: Berlin, Heidelberg, New York.

Dingerkus, Guido (Director, Natural History Consultants, Goshen, New York, USA), 1987 "Shark Attack in the United States". *Sharks*. Consulting Editor, Dr. John D. Stevens (Senior Research Scientist, Division of Fisheries, CSIRO Marine Laboratories, Hobart, Tasmania, Australia) Facts on File: New York and Oxford.

Duchêne, V

1997 *Inleidende studie van de locomotie van Pan paniscus: de bonobo als model voor de evolutie van menselijke bipedalie*. License thesis, University of Antwerp.

Eckert, Robert, 1988 *Animal Physiology: Mechanisms and Adaptations* (3rd edition) by Roger Eckert (University of California, Los Angeles), with Chapters 13 and 14 by

David Randall (University of British Columbia), revised in part by George Augustine (University of Southern California). W.H. Freeman and Company: New York.

Edmonds, Carl, Christopher Lowry, and John Pennefather, 1992 *Diving and Subaquatic Medicine*, Third Edition.

Elsner, Robert (Professor of Marine Science, University of Alaska) and Brett Gooden (Adelaide, South Australia. Formerly Lecturer in Physiology, University of Nottingham) 1983 *Diving and Asphyxia: A comparative study of animals and man*. Cambridge University Press: Cambridge.

Epstein, Murray, 1984 "Renal, endocrine, and haemodynamic effects of water immersion in man", *Contributions to Nephrology* vol. 41, *Cardiocirculatory Function in Renal Disease*, pp. 174-188.

Epstein, Murray, Edgar Haber, and Richard Re, 1978 "A Kinetic Assessment of Aldosterone Responsiveness in Secondary Hyperaldosteronism and in Anephric Man", in *The Endocrine Function of the Human Adrenal Cortex* vol. 18, pp. 493-508.

Environmental and Metabolic Animal Physiology 4th edition, 1991. C. Ladd Prosser, ed. Wiley-Liss: New York.

Frey, William H., Ph.D., with Muriel Langseth, 1985: 147. *Crying: The Mystery of Tears*. Minneapolis: Winston Press

Frey, William H., Ph.D., Denise DeSota-Johnson, B.A., and Carrie Hoffman, B.S.; and John T. McCall, Ph.D., 1981 "Effect of stimulus on the chemical composition of human tears". *American Journal of Ophthalmology* 92:559-567.

Fitch, W. Tecumseh, 2002, "Comparative Vocal Production and the Evolution of Speech: Reinterpreting the Descent of the Larynx". In *The Transition to Language*, Alison Wray, editor. Oxford: Oxford University Press.

Ganong, William F., M.D. (UCSF), 1993 *Review of Medical Physiology* . Appleton and Lange: Norwalk, Conn. 16th Edition. (chart on pg. 343)

Goodall, Jane
[on chimp swimming ability](#)

Hardy, Sir Alister, 1977 "Was there a *Homo aquaticus*?", article originally appeared in *Zenith*, 1977, vol. 15(1):4-6. Reprinted in 1982 *The Aquatic Ape* by Elaine Morgan, Stein and Day: Briarcliff Manor, N.Y.

Hauser, Marc D. and W. Tecumseh Fitch, 2003, "What Are the Uniquely Human Components of the Language Faculty? In *Language Evolution: The States of the Art*, M.H. Christiansen and S. Kirby, editors. Oxford University Press.

Hiraiwa-Hasegawa, Mariko; Richard W. Byrne, Hiroyuki Takasaki, and Jennifer M.E. Byrne, 1986, "Aggression toward Large Carnivores by Wild Chimpanzees of Mahale Mountains National Park, Tanzania", pp. 8-13, in *Folia primatologica*, volume 47.

Hrdlicka, Ales quoted in [Dry Bones](#), Chapter 4."

Hughes, Roland, BSc (former editor, *Australian Natural History*, Sydney, New South Wales, Australia), 1987 "Shark Attack in Australian Waters". *Sharks*. Consulting Editor, Dr. John D. Stevens (Senior Research Scientist, Division of Fisheries, CSIRO Marine Laboratories, Hobart, Tasmania, Australia) Facts on File: New York and Oxford.

Hunt, Kevin D

1994 "The Evolution of Human Bipedality: Ecology and Functional Morphology" in *Journal of Human Evolution* 26: pp 183-202.

Jerison, HJ, 1973, *Evolution of the brain and intelligence*. Academic Press, New York.

Kent, George C. (Department of Zoology and Physiology, Louisiana State University, Baton Rouge, Louisiana), 1978 *Comparative Anatomy of the Vertebrates* (4th edition). C.V. Mosby Co.: Saint Louis.

King, Judith E., 1983:143, *Seals of the World*; British Museum (Natural History), Oxford University Press

Kitcher, Philip, 1982 *Abusing Science: The Case Against Creationism*. The MIT Press: Cambridge, Mass. and London, England.

Kortland, A.; pp. 44-46, in *Grzimek's Animal Life Encyclopedia* vol 11: Mammal II Dr. Bernhard Grzimek, ed. Van Nostrand Reinhold: New York, Cincinnati, Toronto, London, Melbourne.

J. Krüger, J. Mikoleit, H. Heck

2000 "The influence of total body shaving on performance and lactic acid behaviour in swimming", in *Deutsche Zeitschrift Fur Sportmedizin*, Vol. 51, No. 2

Kuliukas, Algis

2001 *Bipedal Wading in Hominoidea past and present*. M. Sc. dissertation, University College London.

2002 (November) newsgroup statements ["...certainly changes my perspective somewhat."](#) and ["...although I feel this data only adds still further to the plausibility of wading as a model for bipedal origins."](#)

2003 newsgroup statement ["the one place apes are pretty much *always* bipedal is in water"](#)

2003 newsgroup statement ["Extant apes are pretty much 100% bipedal in water..."](#)

2004 newsgroup statements on his AAT/H definition, for instance: ["The hypothesis that water has acted as an agent of selection..."](#) and ["To be fair to Hardy and Morgan..."](#)

2004 newsgroup statement ["Stop waffling and let's get back to the point..."](#)

2004 newsgroup statement ["our bipedal locomotion is not suited for woodland, dense bush or tall grassland"](#)

2004 newsgroup statement (on his thinking that I must have made a "mistake" when I reported both pro and con information on hair and swimming speed) ["...one he put in as an own goal by mistake?"](#)

2004 newsgroup statement ["...even though we can swim but chimps can't."](#)

2004 newsgroup statement ["Put it this way, really what's the explanation that chimps can't swim?"](#)

2005 newsgroup statement ["I never claimed it was impossible for chimps to swim..."](#) his site: <http://www.riverapes.com/> [River Apes \(Algis Kuliukas' pro-AAT/H site\)](#)

Ling, John K. (Antarctic Division, Department of External Affairs, Melbourne, Australia), 1965 "Functional significance of sweat glands and sebaceous glands in seals", *Nature* November 6, 1965, 208(5010):560-562.

Ling, John K., 1974 "The Integument of Marine Mammals", in *Functional Anatomy of Marine Mammals*, Volume 2, edited by R.J. Harrison. Academic Press: London, New York, San Francisco.

Marcincuk, Michelle C., MD and Peter S. Roland, MD [Geriatric hearing loss](#): Understanding the causes and providing appropriate treatment"

Martin, RD, 1984, "Body size, brain size and feeding strategies" in *Food acquisition and processing in primates*. Chivers, D; Wood, B; Bilsborough, A, eds. Plenum Press, New York.

Marvin, Ursula B. , Ph.D., 1973 *Continental Drift: The Evolution of a Concept*. Smithsonian Institution Press: Washington, D.C.

McFarland, William N.; F, Harvey Pough, Tom J. Cade, and John B. Heiser, 1979 *Vertebrate Life* (3rd edition 1989). Macmillan Publishing Company: New York and Collier MacMillan Publishers: London.

McGraw, Myrtle B., 1939, "Swimming behavior of the Human Infant", *Journal of Pediatrics*, pp. 485-490, volume 15.

Milder, Benjamin, M.D., 1987 "Chapter 2: The lacrimal apparatus", in *Adler's Physiology of the Eye: Clinical Application*, edited by Robert A. Moses, M.D. and William M. Hart, Jr., M.D., Ph.D. Eighth edition. The C.V. Mosby Company: St. Louis, Washington, D.C., and Toronto.

Milton, Katherine

2000 pg. 1587, *American Journal of Clinical Nutrition*, Vol. 72, No. 6, 1586-1588, December 2000

Montagna, William, and Paul F. Parakkal, 1974 *The Structure and Function of Skin, Third Edition*. Academic Press: New York and London.

Morgan, Elaine, 1972, *Descent of Women*. Souvenir Press: London.

Morgan, Elaine, 1982, *The Aquatic Ape*. Souvenir Press: London.

Morgan, Elaine, 1990, *The Scars of Evolution*. Souvenir Press: London.

Morgan, Elaine, 1994, *The Descent of the Child*.

Morgan, Elaine, 1997, *The Aquatic Ape Hypothesis*. Souvenir Press: London.

Morgan, Elaine

Jan 25, 1996 newsgroup statement misunderstanding convergent evolution and suggesting a new term instead of convergent evolution: ["...but no word for the striking resemblance between the coats of those unrelated mammals. I suggest "paramorphic".](#)

Aug 01, 1995 newsgroup statement misunderstanding convergent evolution: ["White coats in winter are a single feature of Arctic animals which do not in other respects grow to resemble one another"](#)

Feb 01, 1996 newsgroup statement misunderstanding convergent evolution: ["It is just that single shared feature that they have all evolved, and there ought to be a word for this."](#)

Oct 11, 1995 newsgroup post complaining about the use of "hypothesis" in regard to the AAT/H: ["I've noted two new anti-AAT ploys. One is to rename it AAH or AAS...Renaming it is an attempt at philosophical pseudo-speciation, implying "What you are doing is qualitatively quite different from what we are doing.""](#)

Morton, Glenn [Original Morton's Demon essay](#).

Negus, Sir Victor, 1965 *The Biology of Respiration*. E&S Livingstone Ltd.: Edinburgh and London.

Negus, Sir Victor, 1958 *The Comparative Anatomy and Physiology of the Nose and Paranasal Sinuses*. E&S Livingstone Ltd.: Edinburgh and London.

Nishida, Toshisada, 1980, "Local Differences in Responses to Water among Wild Chimpanzees", pp. 189-209, in *Folia primatologica*, volume 33.

Nishimura, Takeshi; Akichika Mikami; Juri Suzuki; and Tetsuro Matsuzawa, 2003, "Descent of the larynx in chimpanzee infants". Proceedings of the National Academy of Sciences. 2003 Jun 10;100(12):6930-3 [online copy](#)

The Open University Alumni On-line Community, *Open Eye On-line*, "Reindeer" December 2000 web.archive.org [copy of the Open Eye Online story](#)

Ortiz, Rudy, 2001, "Osmoregulation in Marine Mammals", in *The Journal of Experimental Biology* vol. 204, 1831-1844.

Paul, Larry J. , BSc (Hons.) (Fisheries Research Division, Ministry of Agriculture and Fisheries, Adelaide, South Australia), 1987 "Shark Attack in New Zealand". *Sharks*. Consulting Editor, Dr. John D. Stevens (Senior Research Scientist, Division of Fisheries, CSIRO Marine Laboratories, Hobart, Tasmania, Australia) Facts on File: New York and Oxford.

Peaker. M., and J.L. Linzell (Dept. of Physiology, Agricultural Research Council, Institute of Animal Physiology, Babraham, Cambridge), 1975 *Salt Glands in Birds and Reptiles*. Monographs of the Physiological Society No. 32. Cambridge University Press: Cambridge, London, New York, Melbourne.

Pond, Caroline M., 1978, "Morphological Aspects and the Ecological and Mechanical Consequences of Fat Deposition in Wild Vertebrates", pp. 519-570, in *Annual Review of Ecology and Systematics*, volume 9; edited by Richard F. Johnston, Peter W. Frank, and Charles D. Michener.

Pond, Caroline M. (Ph.D., Senior Lecturer in Biology at the Open University in Milton Keynes), 1987 "Fat and figures", *New Scientist* 4:62-66.

Pond, Caroline M. (Ph.D., Senior Lecturer in Biology at the Open University in Milton Keynes), 1987 "Not an aquatic ape -- just an exceptionally fat mammal", *New Scientist* 4:64-65.

Pond, Caroline M., 1987, "The great ape debate", *New Scientist* 12:39-42.

Pond, Caroline M., 1987, "The Anatomy of Adipose Tissue in Captive *Macaca* Monkeys and Its Implications for Human Biology", *Folia primatologica* 48:164-185.

Pond, Caroline M. 1991 "Adipose Tissue in Human Evolution", pp. 193-220. *The Aquatic Ape: Fact or Fiction?* Edited by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds. Souvenir Press: London.

Pond, Caroline M. and Malcolm A. Ramsay (Department of Biology, University of Saskatchewan, Saskatoon). 1992 "Allometry of the distribution of adipose tissue in Carnivora", *Canadian Journal of Zoology* 70:342-347.

Poole, Trevor, ed, 1987 *The Care and Management of Laboratory Animals*. Longman Scientific and Technical: Harlow, Essex

Pooley, A.C. (Tony) (Consultant on Crocodile Farming, Conservation, and Education, Scottburgh, South Africa), 1989 "Behavior and Environment: Food and Feeding Habits". *Crocodiles and Alligators*, Consulting Editor, Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) Facts on File: New York and Oxford.

Pooley, A.C. (Tony) (Consultant on Crocodile Farming, Conservation, and Education, Scottburgh, South Africa), Tommy C. Hines (Consultant on Alligator and Crocodile Management, Florida, USA), and John Shield (Veterinarian, Cairns, Australia), 1989 "Crocodilians and Humans: Attacks on Humans". *Crocodiles and Alligators*, Consulting Editor, Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) Facts on File: New York and Oxford.

Pooley, A.C. (Tony) (Consultant on Crocodile Farming, Conservation, and Education, Scottburgh, South Africa) and Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA), 1989 "Behavior and Environment: Mortality and Predators" *Crocodiles and Alligators*, Consulting Editor, Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) Facts on File: New York and Oxford.

Riede, Tobias and Rod Suthers, "Laryngeal descent is not uniquely mammalian" [online copy](#)

Riedmann, Marianne, 1990 *The Pinnipeds: Seals, Sea Lions, and Walruses*. University of California Press: Berkeley, L.A., and Oxford.

Riedmann, Marianne, 1990 *Sea Otters*. Monterey Bay Aquarium: Monterey, California

Ross, Charles A. (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) and Dr. William Ernest Magnusson (Research Scientist, Instituto Nacional de Pesquisas de Amazonia, Brazil), 1989 "Evolution and Biology: Living Crocodilians". *Crocodiles and Alligators*, Consulting Editor, Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) Facts on File: New York and Oxford.

San Diego Zoo

[on chimp swimming ability](#)

Scheffer, Victor B., 1962 *Pelage and Surface Topography of the Northern Fur Seal*. North American Fauna, Number 64, U.S. Fish and Wildlife Service.

Schmidt-Nielsen, Knut; 1964, *Desert Animals: physiological problems of heat and water*. Oxford University Press.

Schoenemann, P. Thomas, 2004 "Brain Size Scaling and Body Composition in Mammals", *Brain Behavior Evolution* 63:47-60

Scholander, Walters, Hock, and Irving (I'm missing the ref for this one -- sorry -- will get it later)

Sharp, Rick L., and David L. Costill, "Influence of body hair removal on physiological responses during breaststroke swimming", *Medicine and Science in Sports and Exercise* 1989: 21(5), pp. 576-580.

Slijper, Everhard J. (Professor, Zoological Laboratory, University of Amsterdam) and Dr. Dietrich Heinemann (Director, Zoological Garden, Munster), 1984 "Whales", in *Grzimek's Animal Life Encyclopedia* vol 11: Mammal II Dr. Bernhard Grzimek, ed. Van Nostrand Reinhold: New York, Cincinnati, Toronto, London, Melbourne.

Slijper, Everhard J. (Professor, Zoological Laboratory, University of Amsterdam), 1979 *Whales* (first pub 1958; revised 1962). Hutchinson of London: London.

Smith, Anthony, 1985 *The Body*.

Sokolov, V.E., 1982 *Mammal Skin*. University of California Press: Berkeley.

Springer, Victor G. (curator in the Department of Vertebrate Zoology, Division of Fishes, National Museum of Natural History, Washington, D.C.), and Joy P. Gold

(Technical Information Specialist at the Department of Vertebrate Zoology, National Museum of Natural History, Washington, D.C.) 1989 *Sharks in Question: The Smithsonian Answer Book* Smithsonian Institution Press: Washington, D.C. and London.

Taylor and Rowntree in 1973 (*Science* 176: 186-187)

van Haeringen, N.J., Ph.D, 1981 "Clinical Biochemistry of Tears". *Survey of Ophthalmology* 26(2):84-96.

Verhaegen, Marc

newsgroup claim ["some/many show undeniable indications of frequent diving \(their ear exostoses\)."](#)

newsgroup claim ["Their ear exostoses leave no other choice... "](#)

[Marc explains to a supporter in his Yahoo group that his affiliation listed on many of his papers, "Studiecentrum Antropologie", is not a real institution.](#)

May 8, 2004 sci.anthropology.paleo ["When one gradually shifts to drinking small frequent bits of seawater + eating fish, it's possible to survive for months."](#)

Feb 22, 1999 sci.anthropology.paleo ["Male Steller sealions have neck hairs, but are otherwise furless."](#)

Dec 14, 2002 sci.anthropology.paleo [on "furless" male Steller sea lions](#)

Dec 27, 2002 sci.anthropology.paleo [on "furless" male Steller sea lions](#)

in his Yahoo group Nov 15, 2004 ["but mammals that spend some time outside the water in cold regions retain fur, except the very large adult male Steller sealions, walruses and elephant seals."](#)

Dec 30, 2002 sci.anthropology.paleo ["Abundant eccrine thermoactive sweating is only seen in humans & sealions on land." and "...furseals are the only non-human mammals which sweat thermoactively through abundant eccrine glands ..."](#)

Dec 28, 2002 sci.anthropology.paleo ["Thermoactive eccrine sweating is abundant in sealions & humans on land. I have no examples of this in other mammals."](#)

Dec 25, 2003 sci.anthropology.paleo "most-sweating" sea lions:

Dec 25 2003 sci.anthropology.paleo ["nonsense... Don't you know, bk, that the most sweating mammals known are furseals on land"](#)

Jan 17, 2004 sci.bio.evolution ["The most-themoregulatorily-sweating mammals besides humans are sea-lions at the shore..."](#)

Feb 7, 2003 sci.anthropology.paleo ["In other words, humans have a normal temperature resembling that of sea mammals, lower than most terrestrial ones, and markedly lower than that of any active savannah species."](#)

Jan 11, 1999 sci.anthropology.paleo [Ear exostoses, Anne, are only found in populations that dive \(usu. for shellfish\)](#)

May 11 1998 sci.anthropology.paleo ["We have the ear exostoses \(only seen in long-term cold water divers\)..."](#)

May 4 1998 sci.anthropology.paleo ["The presence of AEs proves diving habits, their absence does not."](#)

Apr 30 1998 sci.anthropology.paleo ["The auditory exostoses in Ns prove that those people frequently dived in water colder than 19°C."](#)

Jan 10 1999 sci.anthropology.paleo ["Auditory exostoses, bony swellings of the ear canal, a condition well-known to otolaryngologists, occur exclusively as a direct result of long-term exposure to relatively cold water..."](#)

Apr 3 and 28 1998 sci.anthropology.paleo ["\(as Otto Hauser remarked before the soft tissues fell apart when he dug up the Moustier fossil in 1908\)."](#)

Apr 28 1998 sci.anthropology.paleo ["...soft tissues can often still be discerned after unearthing, in the seconds or minutes before the soft tissues fall apart..."](#)

Jun 26 1998 sci.anthropology.paleo ["...you should have known that IMO an external nose arises only \(& not necessarily\) when a mammal starts becoming aquatic..."](#)

Dec 5 2001 sci.anthropology.paleo ["*I* didn't come up with "snorkels" here. Some antiAATer did. No doubt, to ridicule AAT instead of discussing it."](#)

Nov 1 1998 sci.anthropology.paleo ["And I know, of course, when I speak about, say, Neandertal snorkels..."](#)

May 4 1998 sci.anthropology.paleo ["All elephants & proboscis monkeys use the nose as a snorkel to cross rivers or to swim."](#)

"In a Neandertal swimming on his back, the large nose with distal nostrils and the protruding midface surrounded by large air sinuses functioned as a snorkel." pg. 110, 1991 Marc Verhaegen, "Aquatic ape theory and fossil hominids", in *Medical Hypotheses* 35: 108-114

["I once read it somewhere, and I believe everything what I read, at least until I find evidence of the opposite."](#)

Videan, Elaine N. and W.C. McGrew

2002 "Bipedality in Chimpanzee (*Pan troglodytes*) and Bonobo (*Pan paniscus*): Testing Hypotheses on the Evolution of Bipedalism" in *American Journal of Physical Anthropology* 118:184-190.

Wilson, E.W.; and P.I.C. Rennie, 1976, *The Menstrual Cycle*, pg. 36. Lloyd-Luke (Medical Books) Ltd: London.

Wood Jones, Frederick; 1929, *Man's Place Among the Mammals*, (quote on pg. 296). Edward Arnold: London.

[Yale New Haven Health Library](#) "Hearing loss."

Yu-Chong Lin (Department of Physiology, John A. Burns School of Medicine, University of Hawaii, Honolulu, Hawaii), 1982, "Breath-hold Diving in Terrestrial Mammals" in *Exercise and Sport Sciences Review*, vol. 10, pp. 270-307.

Various authors, 1991 *The Aquatic Ape: Fact or Fiction?* Edited by Machteld Roede, Jan Wind, John M. Patrick and Vernon Reynolds. Souvenir Press: London.

Various editors and contributors, 1989 *Crocodiles and Alligators* Consulting Editor, Charles A. Ross (Museum Specialist, Department of Vertebrate Zoology, National Museum of Natural History, Smithsonian Institution, Washington, D.C., USA) Facts on File: New York and Oxford.

Various editors and contributors, 1987 *Sharks*. Consulting Editor, Dr. John D. Stevens (Senior Research Scientist, Division of Fisheries, CSIRO Marine Laboratories, Hobart, Tasmania, Australia) Facts on File: New York and Oxford.

Various authors, 1985 *Social odours in mammals*, Volumes 1 and 2, edited by Richard E. Brown and David W. MacDonald, Claredon Press: Oxford.

Chopard, Stephane, "The Secret World of the Proboscis Monkeys", film (TV program).

Section 2: other references used in making this site.

Berta, Andras, 1992, *Enzymology of the Tears*. CRC Press: Boca Raton, Ann Arbor, London, and Tokyo.

Coughlan, J.P.; B.A. Scoggins, and E.M. Winter, 1979, "Aldosterone", in *Hormones in Blood, third edition, volume 3*, edited by C.H. Gray and V.H.T. James. Academic Press: London, New York, San Francisco.

Darwin, Charles, 1873, *Expression of the Emotions in Man and Animals*. John Murray: London.

Denton, Derek; et al., 1995, "The effect of increased salt intake on blood pressure of chimpanzees", pp. 1009-1016, in *Nature Medicine*, volume 1, number 10 (October).

Elsner, Robert; James A. Willis, Walter H. Inge, and Nello Pace, 1971, "Cardiovascular Responses to Asphyxia in the Monkey", pp. 105-109, in *Experimental Physiology*, volume 1.

Frandsen, R.D., 1986, *Anatomy and Physiology of Farm Animals*, fourth edition. Lea and Febiger: Philadelphia.

Guggisberg, C.A.W., 1972 *Crocodiles: Their Natural History, Folklore, and Conservation*. David and Charles: Newton, Abbot.

Hancock, W.; A.G.R. Whitehouse, and J.S. Haldane, 1930, "The Loss of Water and Salts through the Skin, and the Corresponding Physiological Adjustments", pp. 55-59, in *Proceedings of the Royal Society of London, Series B*, volume 105.

Johanson, Don C.; Maurice Taieb, B.T. Gray, and Yves Coppens, 1978, "Geological framework of the Pliocene Hadar Formation (Afar, Ethiopia) with notes on paleontology including hominids", in *Geological Background to Fossil Man*, edited by Walter W. Bishop. Scottish Academic Press, University of Toronto Press.

Katch, F.I.; and Deborah L. Spiak, 1984, "Validity of the Mellits and Cheek method for body-fat estimation in relation to menstrual cycle status in athletes and non-athletes below 22 per cent fat", pp. 389-396, in *Annals of Human Biology*, volume 11, no. 5.

Milder, Benjamin; and Bernardo A. Weil, eds., 1983, *The Lacrimal System*. Appleton-Century-Crofts: Norwalk, Conn.

Matsuura, D.T.; and G.C. Whittow, 1974, "Evaporative heat loss in the California sea lion and harbor seal", pp. 9-20, in *Comp. Biochem. Physiol.*, volume 46A. Pergamon Press.

Montagna, William; and Richard A. Ellis, 1963, "New Approaches to the Study of the Skin of Primates", pp. 179-196, in *Evolutionary and Genetic Biology of Primates, Volume 1*, edited by John Buettner-Janusch. Academic Press: New York and London.

Prosser, C. Ladd, ed., 1973, *Comparative Animal Physiology*, third edition. W.B. Saunders Company: Philadelphia, London, Toronto.

Schmeck, Harold M. 1995, "Study of Chimps Strongly Backs Salt's Link to High Blood Pressure", in *The New York Times Medical Science* section, Tuesday, October 3. Report on Derek Denton's studies of chimps showing similar chimps' salt responses to be similar to humans.

Ruckebusch, Yves; Louis-Philippe Phaneuf, and Robert Dunlop, 1991, *Physiology of small and large animals*. B.C. Decker: Philadelphia, Hamilton.

Ubels, John L.; K. Keven Williams, Dolores Lopez Bernal, and Henry F. Edelhauser, 1994, "Evaluation of effects of a physiological artificial tear on the corneal epithelial barrier: Electrical resistance and carboxyfluorescein permeability", in *Lacrimal Gland, Tear Film, and Dry Eye Syndromes*, edited by David A. Sullivan. Plenum Press: New York and London.

Weiner, J.S.; and K. Hellman, 1960, "The Sweat Glands", pp. 141-____, in *Biological Reviews of the Cambridge Philosophical Society, volume 35*, edited by H. Munro Fox. Cambridge University Press.

Weiss, Leon, ed., 1988, esp. 562-569, *Cell and Tissue Biology: a textbook of histology, sixth edition*. Urban and Schwarzenberg: Baltimore, Munich.

Pro and Con Excerpts from Feedback

All these excerpts are from feedback emails, which I've sorted into pro and con regarding my site. All typos and grammar are as received. In a couple places I took out potentially identifying info, such as name or a class.

Pro

I appreciate the light you have shed on this topic, and the common misconceptions and "Bad Science" surrounding it. Keep up the good work ! I will attempt to never again support such theories in the future so uncritically.

Nice to see this site back up. It is an extremely good place to refer people to.

I would like to say that I really liked your website about the AAT. I heard of the AAT just a few days ago out of a book titled "Losing Faith: A Quest for Truth and the Origins of Life". When I first read it, I found it rather fascinating, but after I visited your site, I found it to be bunk, so thanks for the cold hard facts.

Your AAT site is a thing of beauty and a joy forever. Now

Con

I have been reading some of the nonsense you wrote re. "the Aquatic Ape I think you are full of what makes the grass grow green.

[NOTE: this was the complete message]

It is a shame that you have spent so much effort in such a negative and destructive pursuit.

Couldn't you have shown some sensitivity? What kind of person are you?

You are all wet.

[NOTE: this was the complete message]

I study this sort of subject matter independently, and have no stake or interest in one side or another being "right." While I am basically only curious about the topic, I find many of your arguments

when I argue with other interested laypeople, I have all the facts I need -- or a resource I can point them to so they can make up their own minds. The amount of work you have evidently put into researching, thinking, and writing is astounding, and you deserve all the brownie points that science and reason can bestow.

Thank you again, for all the work you put into your presentation, and for the several happy hours I just spent at your site.

Thank you, thank you, thank you for your most informative website!!

Your work already has been a great light in a large morass.

Your site on Ms. Morgan and the aquatic ape 'theory' is one of the finest pieces of work on the web that I have come across.

very unsatisfying.

I have been reading some of your presentation. I cannot say I agree with you. I am in your shoes in that I have no degree or formal training in anthropology or the like. I have however looked at some of the six billion odd examples surrounding me. I have also read Elaine Morgan's books and found them clear and logical in explaining how we came to be this way. About 8Ma +or Minus a few million years we were apparently a modest ape from which gorillas and chimpanzees separated and we developed each its own way. The gorillas and chimps apparently developed as terrestrial beasts. However humans certainly must have received their development under much different circumstances. Sir Alister Hardy a Marine biologist suggested we had so many aquatic traits that there must have been an aquatic phase in our development. IMHO there must certainly have been such a

To begin, I am an undergraduate at UCSB. I am in my senior year as an anthropologist. Earlier in my college career I checked out all of Elaine Morgan's books from our school library and read them. I was quite convinced of their validity until I read sections of your web-page. Thank you for your careful research into the evidence that Elaine Morgan used as fact in her various publications.

I read *The Scars of Evolution* last year and found it plausible. I saw a refutation online later and could accept that it wasn't as plausible as I thought, but the refutation was pretty scanty. Tonight I saw a refutation on the Straight Dope site that was better, and it had a link to your site, which was devastating. I'll move Morgan's book to my pseudoscience shelves. Thanks for all the good work!

You've really provided some

phase and to think otherwise is nonsense.

So much for Sir Alister. but you did have a fat head when born.

[NOTE: this was the complete message]

I tried to get to your argument. All I was able to find was your whining about the lack of qualifications of the theory and your own lack of qualifications. I am disgusted by your apology and the random, self-righteous tenor of your response to this issue. Please make sense or get off the internet.

Personally, I find all of it pretty hard to believe - with or without references. After all, aren't all humans fallible? Aren't all scientists biased towards their own work? Everyone can find references and quotations to support their theory, no matter how mad cap it is. Finding flaws in references is no big deal, because the references were probably flawed in the first place.

great information on your site...very logical and convincing.

Thank you for all your efforts to seek the truth regarding the aquatic ape 'theory'. You have saved me hundreds of hours of personal research needed to answer intelligent people's questions about this hypothesis. I belong to 2 science lists and they have both - a year apart now asked questions which I would have been struggling to answer without your site.

I would like to applaud your efforts and the time you have spent on your site.

Read nearly everything on your website...it's very interesting...a healthy debate...

I only heard about the Aquatic Ape Theory a few days ago and (although you must have trouble believing this by now) thought the initial claims gave the idea a certain plausability. I

i went to your hair section first to see if you had an ounce of anything credible to add to the AAT theory.

apparently not.

If your words weren't more nauseatingly pretentious I would have laughed my way through your justifications for sound reasoning, and novel about why theories ought to be questioned. Researching a new subject, the name Elaine Morgan is new to me, but the term "poisoning the well" certainly is not. Perhaps you might display some aptitude in writing for music and film critiques, as far as research is concerned such an outright bias and lengthy plea to the jury does not sound like fair representation of information. With such emotional involvement and defensiveness one may think you are harbouring gills! Just thought I'd drop a line..

might have just filed this away as an intriguing idea that science was taking seriously but fortunately I found it so intriguing that I thought I'd check it out.

It's clear how long you've spent trying to be scrupulously fair to these people and I thought I'd just drop a note of encouragement.

I've just read your site on the AAT/H. Bravo! I had wondered about the status of this theory as I read (and was impressed) by a summary of it many years ago, but I wondered why it was never mentioned by "mainstream" popular science despite the steady flow of books on it I saw being published. Congratulations on such a painstaking, largely even-handed critique.

I was impressed with the job you did on the aquatic ape theory, especially what you seem to know about predation.

I just read your AAT web site. You are one sick and angry puppy.

And just plain mean.

[NOTE: this was the complete message]

I ran across your nonsense again . If I can remember how I got here I won't do it again

[NOTE: this was the complete message]

Your site is hateful and wrong.

A particular human characteristic, I think, is to have an agenda first and back it up with "facts" and "reasoning" second.

Your site reeks of your having an agenda, and an undisclosed one at that.

Just as creationists moot that god put the fossil record there to test our faith, so you'll argue against AAT. I hope some religions are right and that karma is a true thing. One day you'll choke on your own

I searched the web immediately upon finishing The Scars of Evolution, and found your site. I found Elaine Morgan's argument compelling - IF the facts were as stated. But I was suspicious of the absence of real references. Thanks for confirming my suspicions.

Enjoyed your site - just wanted to get bit more background on some pseudoscientific theories.

I have a Ph.D. in biology and the evolution of man has always been an area of particular interest to me.

When I first read the Decent of Woman, I became, and remain totally enamored with the images of aquatic apes. I recognized that Elaine Morgan's approach to the subject would not inspire confidence in the scientific community.

I would like to thank you for all the work you have done in

poison. And I think you know what I'm talking about.

WHY do you not have the guts to say who you are on your website?

This quote is hardly a statement of your name!

As a note, let me state that I am not the Jim Moore who is a primatologist at UCSD, nor am I the Jim Moore who wrote and cowrote several books on Darwin, nor the Jim Moore who's an anthropologist at CUNY. Although I'm not them, from what I've seen of their work, I wouldn't be insulted to be mistaken for any of them. I hope they feel the same.

I can't find anywhere you state "My name is....."

A very unfair & biased critique of AAT.

The logic of the AAT, for me, is undeniably profound. I've

checking her references and facts. I am astonished with your thoroughness.

Nice site. ** Anyway, I liked the site. Very well written and referenced.

My name is

[NOTE: I deleted the name for privacy] and I found myself at your website today after several hours of alternative science reading. Today was also the first day I had ever heard of the AAT. I wanted to comment on your excellent website, it answered many questions I had to this theory. Very informative and readable, even for a 19 year old college brat to understand. Nothing else really to say other than praise.

Have you ever considered writing a book on this subject? I seldom see someone so well informed about what they are writing about. It was just a

discussed with friends with such passion that I've been told to "shut up already."

I read of a woman who developed a Water Ape Thesis back in the 1950s and was sent unceremoniously packing by the male dominated intelligencia of the time. I believe she then used her thesis as the basis for a children's book which was fairly popular when published.

Recently, perhaps within the past year or so, I caught the tale end of some C-SPAN programing honoring the woman I'm writing about.

Was this Elaine Morgan? The symposium vindicated the plausibility of a theory which was, as you know, ignored by academics for quite some time.

The woman I'm referring to was near the age of 80, charming, and gracious.

Boy are you off the beam

pleasant surprise to see such a nice website.

Thanks for an informative, exceptionally researched sight.

I'm doing an essay for my evolution class at the moment and have found your website incredibly helpful. The topic is aquatic ape theory, but we have to summarise the scientific evidence which doesn't support the theory. Your website has helped me to research and think of some of the things that don't quite add up.

I found your website arguing against AAT while doing some research I'm doing. When I read the thing on the seal sweating I started laughing out loud as I had a similar experience with Verhaegen on his e-mail newsgroup.

I read your web page and it is very well done.

[NOTE: this was the complete message]

I'm sorry i cannot see your Facts as facts they seem more like your own ideas and convulated interpitation of things. Your refusal to see things as they are is probably the reason for your negativity.

I don't know much about this subject, but your responses shout of resentment and sour grapes. Maybe you should attempt to think about why this woman threatens you so much.

I looked at the ones of the strong believers then came the strong non-believers, I did not like either very much, finally I tried to pick the unbiased. Your impressive webside appears to be one of those and so is your introduction. How wrong I was, you may very well advocate scientific truths, but does one needs deception to do that? The repect you did build up expertly now lays deflated in my disappointment. Did I detect a great fear as well?

Your home page is impressive and useful.

It is anyway refreshing to see the theory debunked in this reasoned way, but I only wish some opponent of the AAT had taken it a bit more seriously earlier and done this necessary homework rather than just seeming to disdainfully ignore it. However unlike you I am kind and feel sorry for Elaine Morgan and all the other people who have devoted their lives to mistaken theories. Still for the sake of the truth whatever that may happen to be it had to be done.

I had a very hard time finding con arguments on the AAT. Your site does a terrific job of disproving the AAT with solid facts and counter arguments. If I may venture a bit of a criticism. I think your research and counterarguments stand on their own. It seems you get a bit emotional towards Morgan (I realize she does the same). I think your site would be more

This theory is not a fact, it needs a lot more arguments for to even begin to be a danger. However the AAT bug did fly too smoothly into our psychology to be swatted like an annoying fly.

I'm not a particularly faithful proponent of the AAT theory, but the way you are arguing against it is truly ridiculous. All your points are more about form than depth. I'd like to see you rebut the points of the AAT theory in more depth.

if the theory is so wrong, why are you treading water? When swimming one seas the heavens!

I admire you, Jim, for sticking to your argument despite the evidence. We are, despite your assertions, a shore-adapted animal given our upright posture, flat feet, iodine dependency, infantile blubber, etc., etc. There's no disputing our adaptations for shore life. Adaptation unsuited for the

effective (is that possible? How about more enjoyable) if the cuts were kept to a minimum. Thanks for the enlightenment.

This is great. After taking a course on the Evolution of Human Nature at my liberal arts college (maybe my favorite course here, and I'm a senior English major), a friend of mine who saw some special on the Discovery Channel (I think) challenged me to undercut the argument. Without having seen the documentary or read Morgan's book, I didn't have a clue what to say, and with only one semester studying this stuff I probably couldn't have said much even if I had known more about her theory. Your site amusingly and thoroughly put to rest any doubts I had that our class wasn't let down by not hearing any mention of the hypothesis in class from either my professor or Trivers, Wrangham and others we read and had the good fortune to listen to. My friend, while

"desert", "arctic", "mountainside forest", or wherever.

I'll bet you have water piped into your house.

Your criticism lack an underlying understanding of the timeframe in which evolutionary processes take in effect. I would like to see a similar criticism about the AAT based on academic point of view, instead of an agree journalistic approach. And by the way, check some of the more recent findings in Africa, which actually support the AAT.

[NOTE: this is from someone in Denmark, hence the grammar]

Go die in the street you f___ing joke of a hominid.

[NOTE: this was the complete message; I deleted the letters because I don't want some filter restricting access to my site]

Having parented two children into adulthood, I can easily recognize adolescent angst and impetuosity. I see a lot of that

stubborn, should have a hard time refuting your facts and logic. Also, I always find it refreshing and delightful to read someone mercilessly destroy claims that are made self-righteously and without scientific basis. Thanks, and great job!

Compiling the site was no doubt an utterly thankless task, but you can rest assured that it was highly worthwhile as far as this curious bystander was concerned.

Thanks very much for sharing the information on your web site. It's always a pleasure to find something of substance on the internet, and a well reasoned argument is like a fine wine.

Thanks for your site, I was recently involved in a discussion with a follower of AAT, and your argumentation strengthened my gut feeling that their interpretation is not the right way to go. I only

in your work here. Morgan seems more than willing to be civil and constructive. She makes progress by responding to appropriate criticism. You, on the other hand, remind me of the kid in the child-care center who knocks down the constructions of the other kids, but is unable to build any of his own.

One can respect science and one's elders all at the same time.

While your site offers an interesting opposing view to the AAT, I find some things about your site in general disturbing. Firstly, for a individual who is consistently touting the merits and necessity of proper science you have a amazing tendency to present a biased view throughout your whole site. You are supposed to examine the theory and sources, THEN you make your decision as to the validity of the theory. This

didn't know how far-fetched some of their arguments were.

The only downside to your site is that it contains quite a lot of words, which fends off people who would otherwise be interested, in support of your view. And on points it also gets quite personal, which does not add to the discussion, it mainly deviates.

Other than that: great site, glad I've found it.

I've just had my first encounter with your site and I'd like to say I'm impressed.

Cool site on the aquatic ape hypothesis. :> Where's YOUR book? :>

I'm a grad student in geophysics at the Colorado School of Mines. I was searching for information on the development of the theory of continental drift when I stumbled across your site on

is also how your work should be presented. Your writing throughout consistently hints at the conclusion which you have already made and in many places outright makes fun of the theory. Whether or not the theory deserves to be made fun of is besides the point...

History is going to run over you like a big truck squishes a hedgehog. Enjoy.

get a life!

[NOTE: this was the complete message]

Im not pro AAT or anythings. Just happen to come across your site. You right about yourself that you are not a scientist because your arguments are self serving and the way you argue stinks.

Your website talk to you friend and have him comment on your site.

I have just been reading your

the aquatic ape "theory". While I've not heard of that particular piece of fringe science before, I thought you had a good page on comparisons of AAT (and other fringe science) with Wegener.

Excellent! Found you by accessing crank.net - where you have their best label "ant crank." Have spent an enjoyable - and enlightening - evening reading through your sober, lucid, and temperate responses to AAT aficionados in the person of E. Morgan You have done a huge amount of work and it is deeply appreciated by this reader.

Whew, that was a close one! Fortunately I just happened to find your website first before any others about the AAT. I just read the original "Naked Ape" by Desmond Morris; yes, I know it's very old but it was great! Anyway I was intrigued by the Aquatic Ape theory and wanted to find out more. Superficially it seems to make

website and I felt I had to comment on its tone. Is it entirely necessary to be so vitriolic in your response? I could not help but feel that a cold considered presentation of yourself and criticism of the Aquatic Ape Theory would do a great deal to the weight of your argument. I am not in a position to comment on the factual mistakes of the Aquatic Ape Hypnosis as I am a PhD student in landscape theory. However the tone of your site suggested drunken pub argument that does not do justice to your clearly in-depth rebuttal of the claims of the proponents of the Aquatic Ape Hypnosis.

It not my intention to cause offence so I hope none is taken.

The miracle is that E.M. would BOTHER to respond to your various ignorances. Do you think anybody BELIEVES your "debunking"? Her responses

some sense, and it would be really fun to believe it, but I guess I don't! It's very attractive to anyone who had childhood fantasies about being a mermaid; perhaps this was Morgan's original motivation? Don't be too hard on her, she's trying to protect her innocence...

Anyway I just want to thank you for your honest analysis; that's something which is hard to find these days!

I'm enjoying your article on the pitfalls of AAT, thank you.

Congratulations on your AAT site, which is extremely impressive.

I just paid a return visit to your AAT site. I hadn't seen it for a year or so and I have to say that I think it has come on famously - well presented, well researched and even looking good!

I found your website a

are the most interesting material on the website. By the way, you certainly DO NOT have to tell anyone all the scientists who are not you. You are just a wannabe Amazing Randy. Thank Elaine for lending the only credibility contained on your website.

Prior to reading your website I was one of the savannah advocates and had no idea who was pro or contra whatever. That only became clear reading your writing what is clearly contra! That was in no way made obvious in the beginning as it was presented as science. The appeal to the not so scientific who can so easily be misled showed up in the wrinkled fingers suggestion. At the time it inspired me to point this out in my poem where I let my ducky drown, after all few people take a long bath without using some kind of soap or detergent. Fewer still use seawater. Was that being truthful?

fascinating unraveling of the AAT.

First of all, let me express my gratitude for the excellent work you have done on the AAT, and for making this work available to everyone on your website!

I LOVED the site. I first read about the AAT in an undergrad class at UConn (1985?). I'm not sure what pointed me in the direction of the book *The Aquatic Ape*, but when I read it, I thought the idea was lots of fun and just plain cool. I remember thinking at the time that the book seemed a little less scientific than some of the anthropology textbooks I was reading, but it didn't really matter to me because I was an electrical engineering major - anthropology was just a side interest. It's amazing how much you can theorize about a subject when there's no need to come up with hard evidence. Your site (I've read just about the whole thing) has made me realize how weak the whole

I found your pose specious and feeble in the extreme. You contradict yourself from one paragraph to the next.

If this is the best that anti-AAT proponents can do, then the Aquatic Ape theory is not a theory but rock-solid fact.

I have no doubt that you will think that you can pick holes in the above. The reality is that all advances in human knowledge started as theories. There were all pooh-poohed by the scientific establishment at the time. But as time went by, additional evidence (facts not waffle as yours), came to light. Theories were modified, until they became accepted ... until the next advance. Cf. Flat Earth/Round/sun goes round the earth/ Solar system etc).

Could it be that your antipathy to the AAT is a disguise for a disbelief in evolution?

Please do not bother to respond, your web site has demonstrated your inability to

AAT theory is. It was a cool idea, it would have been nice if there were something more to it.

I came across your website purely by accident while browsing looking for info about breath reflexes.

Even though it didn't provide the info I was looking for, I spent some time reading over various parts and found it interesting, informative and entertaining.

Particularly, the section "My response to Elaine Morgan's response to me" was very entertaining reading.

I feel personal attacks and hostility to constructive criticism are a sign of possible insecurity in EM's belief in her own theories and I just wanted to commend you on the non-inflammatory way your rebuttal was presented.

I have much enjoyed reading

consider objectively any point of view beyond doubt..

I was interested in the issues, but most of your page is spent quacking about the presentation style of a particular show.

you think you know and i think i know, both ignorant and dreaming!

Well, I've used my cranium to think about all this, and I'll go with Sir Alister Hardy--He originally came up with the 'AAT'.

I stumbled across your website recently, and I found myself somewhat disturbed with the manner in which you criticize the aquatic human evolution theory. I could not finish reading your website because I was so disgusted with how much you slander those who have this theory. You are obviously entitled to your right to express your opinion on this

your comments on the arguments propounded in favour of 'the aquatic ape theory'. They have certainly convinced me that the evidence in favour is very suspect.

It is regrettable that, as it seems to me, you make so many comments about the intellectual and moral failings of the proponents of AAT. They are human and therefore fallible, as are we all. I would have expected a rational person to be content with assassinating their myths and to leave judgement of character to those acquainted with the individuals concerned.

I've just read through your intro page on AAT: Sink or Swim? and I wanted first to thank you so much for the website.

Good luck with all your further research, and thanks again for the great website and

matter, but in the same respect they are entitled to theirs, however misguided you feel it is. Your first several pages discuss the lack of credibility in the authors and citations of the theory, and you continue to discuss minor and insignificant inconsistencies in what they write. If you cannot entirely prove your point without the hard, scientific facts, then you give the impression that you yourself are not credible.

Come on whoever you are/
come on lets get real?

The "aquatic ape hypothesis" should be assessed by scientist with reference to factual and theoretical information indicating the pro and con in a balanced way and not by an amateur as you. In raising the awareness of facts not appropriately explained by the established paradigm amateurs have a role to play by seeing what those trained within the paradigm may overlook, but to assess the validity of "aquatic

information, and all the work you've put into it.

Just surfed your site on AAT. I recently saw a "documentary" on the subject that was very soft on science – almost like watching "In Search Of...", but I didn't realize it at the time. I'm taking an anthropology class, and wondered why Elaine Morgan wasn't mentioned at all in the text. Now I know.

I have been reading through your AAT: Sink or Swim webpage, and I have found it to be both informative and interesting.

Congrats on your page, interesting reading and well oriented. Keep up the good work.

Your site was a great find for me because since I first came accross aat I've been looking for good scientific arguments against it, but haven't had time to dig up primary sources myself. I also found Morgan's

ape hypothesis" requires knowledge of scientific methodology which you clearly lack. Therefore you are wasting your own and the readers time.

I have red your critics on the AAT/H theory. There is too much bla,bla,bl, and very little concrete statements. It is obvious, that your critics are not objective, you would even criticize Elaine Morgan hairstyling if it would be mentioned in her theory. I like to see your critics on the official Savannah theory and bipedism.

Sorry to tell you that, but I like so see constructive critics.

lake of referencing to be frustrating, but I guess I let it slide because she was an amateur, and to honest with you, I suppose I wanted the underdog to win.

As an avid surfer who derived much pleasure from the sentimental concept that somewhere in the genetic code there were Amphibian ancestors that explained and justified the inordinate amount of time I have spent in the ocean surfing.

Your logical critique of the Aquatic Theory has painfully scorched my gills. Since I am now at a lost, thanks to you, for explaining my addictive amphibian aspirations, I will now, late in life, face the hard terrestrial facts to go out and find a land based job. I hope you're happy, you land lubber.

Cool website... don't know what to believe and I guess thats why it's so good...

Hello, I was briefly scanning through your aquatic ape website, and found it outstanding. I am a marine biologist myself with a slight knowledge of anthropology. I also belong to the Bad Astronomy message board, a site frequented and run by scientists, not all astronomers.

In one of our non-astronomy discussions, the aquatic ape theory was brought up.

I invite you to drop by, and to contribute if you'd like. Or you may like to hang around and read about the other topics. In any event, I thank you for spending the time in developing such a wonderfully thorough and scientifically rigorous website.

Thanks – I appreciate the amount of work you put into this. I was always tempted to believe in the AAT, but the specific points you make are excellent, and after reading

your website, I have to agree, the AAT is doggie doodoo, and I have put it into the same category in my mind as Bigfoot, and UFO's.

Hi my name is

[NOTE: name deleted by me].I wanted to find killer whales.I COULD NOT FIND THEM! I am nine. I need buttons to find killer whales. please put some buttons on please!!!!

Thank you so much for relieving me of my misconceptions!

Keep up the good work.

You seem to be doing an excellent job of taking apart the Aquatic Ape Theory. I haven't double checked your conclusions yet, but your thoroughness of reference and your value of rigorous criticism seem about right.

I recently read Elaine Morgan's

book "the aquatic ape", and found it extraordinarily convincing. Having read biology at oxford university I was amazed never to have heard of this theory before, and was determined to find out more about it. I started looking on the internet and quickly came across your website, which I have been reading with great interest.

It is clear to me from reading your site that much of the information provided by AAT proponents is misleading or inaccurate, and that it does not provide a balanced view of the evidence. In this respect I feel a little cheated by Elaine Morgan's book, as I found it so convincing when I first read it. However I have also found your own website a little frustrating, as it also seems to have a certain missionary zeal (i.e. that the AAT must be destroyed) and it leaves unanswered what I see as the really key questions.

Enjoyed reading your website -
balanced and informative.

I have been to your site and
your critique gave me a good
feeling for what I had missed.
What you say does seem to
make perfect sense.

Mmmm. I've read your critique
of DAs programmes with
interest.... You denigrate my
childhood hero at your peril!
But true science cannot be
insulted and does not fear
critique. I was impressed with
your web site. I shall read it
again and the rest of your site,
soon!

Thank you for doing the right
thing and exposing bad
science. I applaud the scientist
who goes out on a limb with a
questionable theory. Einstein is
a pretty good example of that.
But Elaine Morgan is clearly
more interested in selling
books than seeking the truth.
She's a novelist who
shamelessly deceives the

public.

I enjoyed your serious coverage of the aquatic hominid hypothesis but was slightly put-off by your need to include a defense of your ability to do it at all and your exchanges with protagonists or supporters of the hypothesis. This left me with the distinct feeling that you initially set out with the purpose of not objectively examining the theory but to refute it. Having this mindset even before you began your research introduces the question of bias and reduces the effectiveness of your critique in the long run.

You have a large site that quite clearly shows how unscientific Elaine Morgan actually is.

I was on your aquaticape site and found it well-written. I also check out many of the AAT "facts" myself and find them interesting, but as yourself

stated - the "facts" are unsupported.

Keep up the good work.

Your website refuting the aquatic ape hypothesis is great.

Hi. Like your site, even though you are destroying some of my favorite beliefs. So it goes.

I just found your site (and spent the next 2 hours reading it!). And an excellent read it is too.

So, thanks for the effort you've put into creating the site, it was much appreciated (and I learnt a lot from it). I will admit that when I first found it I thought "Oh no, another crank site, this guy must be obsessed to have spent all this time on this". But, as I read through it, the more impressed I became - not least with your patience with the AAH lot! And you even made me laugh in a couple of

places in your critique of Morgan's latest book. I laughed so loudly at " I do that sometimes; someone says "boo" and I jump several million years later." that my wife asked me what I was reading (and was rather puzzled by my attempted explanation!).

And, finally, you have even persuaded me to read Darwin. It will do me good to actually read him, after a life time of referring to his works!

I'd like to offer my thanks for your site on AAT. Having read a number of Elaine Morgan's works, I had felt some sympathy for the AAT, but was uncomfortable with the absence of useful criticism. My early web searches produced nothing more than ugly diatribes against Ms. Morgan, which increased her status as martyr in my eyes. Of course, these searches were back in the late 90s, and for some

reason I never came across your site.

So I was greatly pleased when I came across your site, with its detailed refutations of all the major claims of AAT. At last I had an opportunity to see both sides of the debate! The information you offer is compelling, and has shattered any sympathy I had for the AAT. The best kind of learning is that which changes your mind on something, and I offer my appreciation for your providing that.

One suggestion: please tone back the sarcasm and ridicule of Ms. Morgan. You don't need it; your facts demolish her position. The sarcasm and ridicule suggest an emotional involvement that undermines your credibility. The most devastating stance you could take would be a calm, quiet refutation of the arguments themselves. Good writing never draws the conclusions for the

reader; it makes them obvious and lets the reader draw them.

The amount of digging you have done to uncover references is impressive. You really are an anthropologist! I commend you for taking the logical, scientific road.

Thank you for your excellent site

<http://www.aquaticape.org/>

on reading your AAT opus, a standing ovation!

I wanted to let you know that I really appreciate finding your website on the AAT/H, which I have just started looking at. I teach a bunch of courses in the philosophy of biology, and we had a little bit of discussion of the AAT/H (on science vs non-science) last semester in my my

[NOTE: specific course info removed for privacy reasons], but it was

crammed in to a course that was already pretty full, and I

wasn't all that well-equipped to talk about the AAT/H. This semester I'm teaching

[NOTE: specific course info removed for privacy reasons], where I've decided this year to make Donna Haraway's **Primate Visions** one of the texts. And here I think I will be able to work in a more sustained discussion of the view. Since I also am likely to give students the option of writing or modifying a Wikipedia article on some suitable topic as one of their assignments, the link to your site from that article (which I assume you had input to, but I haven't checked) is great.

I've just had the pleasure of reading your website. Absolutely fantastic. I had a reference to one of Elaine Morgan's books at the end of one of my lecture notes for my zoology degree. the book was referred to in a 'and this might be worth a read if you fancy looking at the issue from another angle' way. i thought i would have a look at

the book to put off revision as its water born past tone sounded just like the kind of thing i would really enjoy being a surfer, a closet hippy and someone who loves it when the scientific world is turned on its head. I then decided that reaserching the book online first might save time. Boy am i glad i did. I have never seen a theory so comprehensivly and profesionaly shot down. it was wonderfull to read. So profesionally argued definatly gonna put some of that in my exam.

Wonderful site, please keep up the good work.

I'm impressed by the amount of work that you've put into your site; I must admit I haven't read it all yet but I'll come back to it.

I just read scars of evolution, and was wondering why I had never heard of the aquatic ape theory before. So, I had a look around on the internet. I think

you've done a good job of refuting the theory. Definitely a lot of work, and well done.

thanks a lot...

I have gone through your website, found very informative

Just a quick note to say that I have spent a number of hours today reading through your AAT site

(<http://www.aquaticape.org/>) and it has been an enjoyable and informative experience.

I have just found your site and after many years I am very pleased at last to find someone to answer the many questions raised by the aat. A hard job but someone had to do it.

Thank you for publishing your work on the internet. I had been confused by the AAT until I read your article disclaiming it.

I only have a peripheral

interest in this subject, I loved Elaine Morgan's book Descent of woman far more for what she did with the naked Ape than her own hypothesis, which seemed weak. But I want to thank you for making a huge study of this putting all the information out there for anyone who is interested. You have done a great job, and when I have any questions I know where to come.

hello, nice and interesting web site about the aquatic ape

I just read about your site on aquatic ape theory, and it was great. Well I really skimmed it, since you spent way too much time working on it. Really you could have condensed all this material into a single page, and anyone with half a brain would get the point.

I have found your website very interesting and informative, alas, i can only use journal articles for my dissertation.

Some years ago I heard Elaine Morgan speak at Radcliffe College on the theory, and I was rather taken with it. And when I mentioned it lightly to friends who knew more about such things I was dismayed at how dismissive they were... since they never seemed to address the elements of her argument that seemed convincing. I did not thoroughly read your essays. In fact, I quickly found the points I wanted to know more about in your "Claims and the Facts" section.

Arguments on a species would have evolved X because of Y or wouldn't have always seemed weak to me. But your simple list of claims made by proponents and why they are untrue was informative and welcome.

I am hugely disappointed to read that a theory which felt so right to me does not hold up, but I am grateful for the hard work (and clean organization

and navigation) you put into this.

Intrigued I 'googled' 'aquatic ape' and found your very interesting internet site. While I've not had time to read all of it, it has put me on notice as to the theory and the possible fundamental holes in it.

I'm perfectly happy that you have your opinion, and you're entitled to it, but I still think you're a coniving weasle for choosing the site name aquaticape.org. That should be pretty obvious. You, sir, represent everything bad about the internet.

It is nice to see that somebody is taken time to make a deep analyse of the AAT. Good work! But at the same time I have to confess, I am also one of these people beliving at the potential of the AAT....

Bravo! Your website is masterpiece of rationality

backed up with clear facts and sources.

I just wanted to thank you for your thorough handling of the "Morgan Theory" (<http://www.aquaticape.org/>).

As it happens, I read the first Morgan book when I was 16 or something (25 years ago!) and was immediately fascinated. Even then I understood that this wasn't science but only speculation - but very believable speculation, I thought then.

Now, as I was reading "The Third Chimpanzee" by Jared Diamond, I once again remembered the aquatic ape theory and started wondering if it ever got anywhere. And, thanks to google, found your site. And got a bit wiser, once again.

So, thanks for your research and effort,

Anyway, this was meant to be

a quick note, but since I'm all fired up now, it has become somewhat longer. My apologies. But kudos on the excellent site. It not only addresses the issue in a very fair and honest way, it serves as something of a primer on scientific and logical reasoning, as well as on the importance of doing honest research (rather than cherry-picking quotes). It shows that real science, and real debates, require real work.

I appreciate that. Nice work.

Your site in objection to the AAT is well thought out and executed.

Brilliant stuff, Jim. Seldom have I seen such diligent and persistent skepticism applied to a marginal theory.

Thank you for all your effort put in the creation of this page. I didn't know other terrestrial animals can dive :-).

It was yesterday when I faced for the first time the AAT/H; I must say it had a romantic appeal. It was today when I tried to deep my research on the matter when I happened to read your page. This is just a mail to thank you for all the work you have done; specially, for showing us the sources where to deepen the knowledge on this matter.

Objective and carefully prepared scientific analysis of a "theory" is often hard to find in this day and age, especially on the public forum of the internet.

What I liked most about your site was that you simply do what any of the proponents of this fictional idea could have done - and presented your conclusions based on careful review of the available data and information.

It must be embarrassing for Ms Morgan and her ilk to not be

able to extricate themselves from this idiocy.

I read Desmond Morris years ago, and found his mention of Aquatic Apes amusing. My father is a marine biologist and I know he thought the idea of an aquatic ape evolutionary pathway was hilarious.

Your site is fantastic, I think it actually is missing a worthwhile page or two: all the other, better, actually scientific studies. I know it's a bit of work, and actually moot when dealing with the theory on it's merits, but knowing about Bramble and Leiberan's paper showing how distance running accounts for a lot of modern human physiology and does so in a very scientific and very well examined way, with fossil evidence to back up the claims. Your site really is fantastic, information wise. You could do with a couple pictures though.

I really like this website and

I'm quite impressed by how comprehensive it is. A greater compliment than any I can give, however, is that Prof.

_____ [NOTE: name removed for privacy reasons] included it on his syllabus.

I came across your site when googling "swimming infants", wanting to see if swimming lessons would be of any benefit to my full-of-beans six month old boy. Instead of that info (and yes, that may have been a lame search string for that particular investigation), I ended up having a very enjoyable diversion through your sensible and amusing site. Just wanted to say good on you for doing it, and keep it up.

I just read "The Aquatic Ape" tonight and I admit I was rather taken in by it. Hardy's admission that he was just poisting a speculative hypothesis went a long way toward reasuring me that he was being honest. When I jumped online to check out the

idea I fortunately stumbled on your site first. I like to think Im pretty good at detecting pseudoscience, (the lack of citations did raise a red flag) but I wasn't prepared for her (fairly) good reasoning from simply fraudulent evedence. I think you are right to point out the similarities with ID. Pseudoscience is pseudoscience and fraud is fraud. And they that write it can all kiss my ass.

I have come across you website during my research on sodium. I think you have:

1. great address (aa.org)
2. Great information (loads!)

but:

1. it would be much better if under such a powerful address you could find all these great info more organized and put in a way that is more suitable for online reading.
2. try to make it sound serious and formal.

Well Jim, you clearly don't need it, since you argue with gusto & confidence, but I have to say bloody well done, mate. It is SO good to see an approach which is honest, straight and devoid of the egocentricity which infects science.

Fascinating that you should attract such vicious opposition. It appears that you've blasphemed against a religious doctrine. It's the Flames for you, old chap.

I've read a good way through your stuff, as well Morgan et al, also whatever else I can find on human evolution generally and am left with the conviction that it is deserving to apply the common man's reaction the AAT/H is just plain bollocks.

Not scientific, but the argument is so full of such obvious holes that the BC (Bollocks Conclusion) is inescapable.

I am amazed by your ability to

research this subject so thoroughly and express its failings so succinctly and in such depth.

A friend linked me to a video of Elaine Morgan and her advocacy the aquatic ape theory. I was sceptical at first, and by the 12th minute I decided to see if anyone had done any work on the web analysing it the aquatic ape theory. Unfortunately the majority of websites were as emotion driven and baseless as Elaine Morgan herself. However your website really wowed me - very very good work

I liked the sink or swim site, very thorough.

I've just spent hours reading through your wonderful www.aquaticape.org and really appreciate all the (countless) hours you must have spent dissecting, explaining and covering all aspects of AAT/H and its proponents tactics.

While surfing the internet, I came across a video of Elaine Morgan talking about AAH. I'm just a lowly IT manager, not a scientist, but I do find science interesting. I was very caught up in her speech and after she was done I went straight to Google.

Whoa! When I started reading your site, I found myself reading your writing from a frame of mind that was in defense of Ms. Morgan. I was enthralled with her spirit and plain talk, and was sure that you had to be wrong in your critique.

However, it didn't take long for my position to change.

I read through your test of the aquatic ape hypothesis on your website with delight.

I am a retired Natural History Museum research scientist and still Professor at [NOTE: university

name removed for privacy reasons]

University (marine biology).

I thought the AAH had died out years ago. Believers never let evidence change their mind.

It is nice to see a layperson with so sound a grasp of the scientific method - and of logic.

I've just spent a good chunk of time reading your excellent site, and thought I'd send you a note of appreciation.

That is simply a great job on laying out the science - and well presented in a very clear manner.

I just want to say thank you for your work on aquaticape.org

I consider myself a skeptic, and I value science and critical thinking, so I had to slap myself a little after I looked at your website. Because I had watched Elaine Morgan's talk at TED, and found it great. The whole 'conspiracy' theme

should have been a red flag,
but I did not see it.
aquaticape.org has been not
only a very useful resource for
learning about AAT, but also a
reminder of how gullible one
can be.

I just wanted to say "Thanks"
for setting up your website:
"Aquatic Ape Theory: Sink or
Swim".

Links

[Stephen's Guide to the Logical Fallacies](#)

A classic source for info on dishonest arguing, and how to recognize it. Several logical fallacies are commonly used by AATers, including False Dilemma, Attacking the Person (argumentum ad hominem), Hasty Generalization, Unrepresentative Sample, False Analogy, Slothful Induction, Fallacy of Exclusion, Subverted Support, Non-support, and the ever-popular Straw Man.

If this link is not working, try Art Russell's page below -- the basic info about the logical fallacies is the same.

[Art Russell's similar page on logical fallacies](#)

[River Apes \(Algis Kuliukas' pro-AAT/H site\)](#)

Also contains his criticisms of my site.

An excellent write-up of the [Fred Hoyle and Archaeopteryx](#) affair in the Talk Origins FAQ.

[Project Gutenberg](#)

Project Gutenberg is one those things the web should be about, offering online, downloadable texts from many public domain books, including most of Darwin's books. A must bookmark.

[The Telson Spur](#)

A collection of links, only more than the sum of its parts. A great place to find science info, info about the web and how to use it, or for when you just want to surf and exercise your mind.